JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

F. H. WEBB, SECRETARY.

VOL. XXII.—1893.

London:

E. AND F. N. SPON, 125, STRAND W.C.

New York:

12, CORTLANDT STREET.

1894.

TABLE OF CONTENTS.

VOL. XXII.

TABLE OF CONTENTS.—No. 103.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII. 1893. No. 103.

The Two Hundred and Forty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 12th, 1893—Professor W. E. AYRTON, F.R.S., Past-President, in the Chair.

The minutes of the Ordinary General Meeting held on December 8th, 1892, were read and approved.

The CHAIRMAN: In the ordinary course the new President would have given his Inaugural Address this evening; but, unfortunately, he is kept by business in the Mediterranean, and is unable to be present. His Inaugural Address will therefore be given at the next meeting, on January 26th.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

F. W. Cooke.	T. L. Miller.
John A. McMullen.	Danl. Sinclair.

From the class of Students to that of Associates—

R. P. Brousson.	Walter C. Goodchild.
J. F. Conradi.	George Alfred Jones.
H. G. E. Cross.	Frederick W. Mills.
H. Justice Glynn.	George Harry Parsons.

George Henry Victor Roller.

Donations to the Library were announced as having been received since the last meeting from the Director-General of Telegraphs in India; Mr. H. R. Kempe, Member; and Professor Silvanus Thompson, Member; to whom the thanks of the meeting were duly accorded.

Mr. H. Human and Mr. H. E. Mitchell were appointed scrutineers of the ballot.

The CHAIRMAN: We will now resume the discussion, which has already been a lengthy one, on Dr. Fleming's paper on "Experimental Researches on Alternate-Current Transformers." Professor Fitzgerald is here, and perhaps he will give us some remarks.

Professor Fitzgerald.

Professor G. F. FITZGERALD: Mr. President and gentlemen,—I am sorry to say I am not practically acquainted with the subject, but I think that it may be worth while to call the attention of makers of transformers to certain conditions under which eddy-currents will be produced. The thing arose in March, 1891, when I wrote a letter to Professors Ayrton and Perry. The letter was written in connection with a discussion on the efficiency of transformers going on then. It seemed to me those engaged in the discussion had not noticed or had not paid attention to a warning which Lord Kelvin had given as to the eddy-currents which would be produced in the thick wire of a transformer if it was sufficiently thick; and I am entirely basing the interest which should attach to the question on the fact that Lord Kelvin at that time asserted in his paper read before the British Association—in 1890 it must have been—that the effect was sufficiently large to be interesting. There are, of course, plenty of effects that are so small that at present, at least, they are not

Professor Fitzgerald.

particularly interesting, though it is well for people to pay attention to things which are now uninterestingly small, but which may after a few years become interestingly large. The letter I wrote was to call attention to the fact that the copper coils of transformers were large masses of unstranded copper, in which eddy-currents could be set up as well as in other masses of copper in the neighbourhood, and that it might be of importance—and Lord Kelvin said was of importance in some cases in which he had calculated the effects—to strand the thick copper wire. Well, one of the points he called attention to was that in the case of a closed magnetic circuit transformer there was no magnetic induction outside the coils, and, consequently, a piece of copper outside the coils will not have electric currents induced in it, while a piece of copper inside the coils will have eddy-currents induced in it; because, unless there is actually magnetic force at the place, it will not induce eddy-currents, although it will induce a current continuously round in a circuit surrounding the coil. This latter is the point upon which Professor Ayrton wrote to me recently in connection with this discussion on transformers to further explain. It is not my point—I am not responsible for it; it is Lord Kelvin's point—he is responsible for it. If you have got an iron ring magnetised by means of coils of wire, the coils of wire being outside the iron, a piece of metal *outside* the coils will have no eddy-currents produced in it. A piece of metal *inside* the coils will have eddy-currents produced in it. That is, although a wire that was wound round completely surrounding the ring would, of course, have the useful current we want induced in it, a wire lying entirely outside, no matter how thick it was, would have no eddy-current induced, no Foucault current induced. If, however, you have a thick wire wound within the region in which there is actual magnetic force, then that thick wire would have eddy-currents produced in it. It would apparently, then, be desirable to have thick wire wound on the outside rather than the inside of the coil. All this is, of course, upon the simple hypothesis that there is no leakage of magnetic lines out of your closed circuit into the regions outside. It is very important, of course, to distinguish

Professor Fitzgerald.

between lines of force which are due to imperfections in your magnetic circuits, and the lines which would exist in case your magnetic circuit were perfect. In the case of a perfect magnetic circuit no quantity of thick wires outside your magnetic field is of importance. There will not be any eddying produced in them. There seems to be a certain amount of difficulty in understanding how it happens that you can have a current induced round this ring, and at the same time no risk of eddying within the material of which the ring is composed. Looking at it from the ordinary point of view, you will see it is quite natural. Take the case of a piece of metal: why are eddy-currents produced in it? If there are lines of induction *within* any piece of metal, there should be a current of electricity induced in the circuit round that field. There are lines of induction passing within its thickness; then you will have a circle of induced current round one side, and back again by the other side. You will have a current up outside the wire, and back again inside the wire. If there are lines of force within the thickness of the copper wire, but if the induction is all due to the lines of force within the circuit of wire, there will not be any difference between the E.M.F.'s on outside surface of the wire and on the inside surface of the wire. It is only the difference between the E.M.F. outside and inside that enables the current to circulate within the thickness of the wire, and there will not be any difference in those forces if both are due to the same sets of lines. Only the difference due to induction within the thickness of the copper itself will produce eddying in the thickness of copper wire. On the outside of the transformer, which has no leakage—which no perfect one ought to have, but which practically all have—no mass of metal will have eddying produced in it. Now the eddying due to this is in general, I expect, comparatively small, because the induction at all points is small, unless it is due to induction leaking out of iron. Induction in the air, which is entirely in the air, produced by a current, is generally comparatively small, so I would not expect that the effect due to this was anything very large. I suppose the thing that is really of interest is the question as to whether, in order to

Professor Fitzgerald.

avoid this eddy effect, you should wind the thick wire on the outside of the transformer or on the inside of the transformer,—whether the thick wire should be wound outside the thin coils or inside the thin coils. Well, in a closed-circuit transformer, such as the simple case which I am taking, in which there is no leakage of the lines, it is on the average, I think, better that the thick coils should be on the outside; but in order to avoid this eddying I think there is no doubt at all that the driving circuit should be on the inside. Whichever is the driving circuit, the thick or the thin one, that, I think, ought undoubtedly to be on the inside. Take the case of a central space to represent iron, draw a line outside through the thickness of the coils, and draw ordinates to represent the intensity of the magnetic force at different points inside the coils. Well, the magnetic induction in the iron will in general, on the average, be the same as the driving magnetic current—that of the driving circuit. Supposing we begin by supposing that the driving coil is outside. Begin outside the outside coil, and the magnetising force is nothing; as we come in through, the magnetising force will increase up to something considerable. The phase of the driving circuit—the magnetic induction due to it—is very nearly opposite in general to the magnetic induction due to the driven one, and consequently the average magnetic induction within the inside coil would diminish, and would be comparatively small when the iron is reached. The magnetic force in the iron is comparatively small, and the induction in the iron is very large. On another diagram we might represent the transformer in which the inside is the driver. In that case you have the magnetic force increasing as before on proceeding inwards from the outer surface of the coils. The driving one must overpower the driven and bring about the same magnetic force as before when the iron is reached. On drawing this diagram it appears that the average magnetic force is very much less than in the former case. The average magnetic force within the region of the coils is comparatively small, and consequently the eddying effect in the copper wires will be very much less in the case of inside driving than in that of outside driving. There is very little doubt, so far as producing eddying

Professor Fitzgerald.

effects in the coil itself is concerned, that it is desirable that the driving coil should be wound inside, and that the driven coil should be wound outside. The next question is, Where should the thick wire be? Well, if you have a thick coil, and insist upon having it inside, you had better transform up rather than down, because the average magnetic force within it is less. This is only as regards the magnetic force which is normally present in a continuous magnetic circuit transformer within the coils not due to leakage. Then comes the question, Supposing you had to take into consideration the leakage effects, which would be best—to put the thick copper wire on the inside or the outside? On the face of it, the leakage of lines of induction out of the iron is denser in the neighbourhood of the iron than it is further away. Lines of induction spread out from the iron in general in the air outside, and consequently in general certainly the density of the lines of force will be greater close to the iron than outside, so that for a given mass of iron or a given mass of copper it would be better to have it as far from the iron as you can. Therefore, so far as leakage of the lines of induction is concerned, it would undoubtedly be best to have a thick wire on the outside. Because, so far as leakage lines of induction are concerned, they would be denser near the iron than outside. Then comes the question, If you do put the thick coil on the outside, there will be more copper in which there will be this eddying effect, and do you thereby increase the heat production in that copper by having more copper? Well, the question is not, I think, capable of being solved with absolute certainty all in a moment, but I think there are certain considerations that make it almost certain that even with more copper it is undoubtedly desirable to have that copper as far from your iron as possible. Let us consider the case of iron out of which there are lines of force leaking and a certain coil of copper wire wound round it, and another case in which you have the same thickness of material wound round a considerably larger circuit through the same arc. Now the most unfavourable case for the wire outside would be a case in which there are, say, three lines of induction passing through the thick wire near the coil, and also three lines passing through the copper which was far

Professor Fitzgerald

from the coil. In general the density is greater near. We are certainly making it as favourable a case for the inside one as possible to suggest that the same lines go through the inside as through the larger outside. You will have a certain E.M.F. during the production of the three lines of induction in the inside copper, and you will have the same E.M.F. tending to drive a current through a very much longer piece of outside copper. As a first approximation, for a given E.M.F., the production of heat is inversely as the resistance of a circuit, and consequently heat-production in this long circuit with a given E.M.F. will be considerably less than the heat-production in this smaller circuit, which has the smaller resistance. Well, that is to a first approximation. Then comes the question, If we have got a hundred, or a thousand, or a million alternations per second, does the same thing hold? Well, it does not hold accurately the same thing at all. But for a hundred alternations per second, or for ordinary transformer work, I think there is very little doubt that the self-induction of these long, narrow circuits, in which you have a current running in one direction, and another running in the opposite direction quite close to it,—the self-induction of that long, narrow circuit will be a small quantity compared with the resistance of that long, narrow circuit, and consequently the first approximation here will be an approximation to the truth; and therefore the conclusion that it is better to put your copper outside, though you have more of it—that it is better to put your thick copper as far as possible from the coil, so as to avoid lines of force leaking—is enforced by this further consideration, that your additional quantity of copper will add rather additional resistance to the circuit than give you more copper in which there will be eddy-currents, and therefore on the whole it is well to keep your thick copper wire as far from your iron as possible.

Supposing you wanted to produce as much heat as possible by an alternating current. I suppose the theorem is very well known. How should you arrange the resistance of a circuit and the self-induction of a circuit? For a continuous E.M.F. the better the conductor the more heat is produced by it. But if we introduce the question of self-induction the whole thing is

Professor Fitzgerald

changed. If I have a *perfect* conductor, and I have an alternating current in its neighbourhood, the only effect will be to produce superficial currents in that perfect conductor which will neutralise the action of the alternating current within the conductor itself; and if we take a case of a circuit of any size, and with a certain amount of resistance and induction in it, and a certain rate of alternation, if the resistance is equal to the self-induction multiplied by the rate of alternation, that is the condition in which you will have maximum heat-production in the circuit if the circuit has a given resistance. Another question bears on the question I mentioned before, of things that are immeasurably small at present and may be immaterial, yet which may at some fnture time be material. I am quite certain that at the present moment this further effect is of no importance, and may at some future time be of importance, and it is well to call attention to the fact that these things may sometimes be of importance. A good number of people may be surprised that there are lines of induction which are starting within that iron ring. How do they come there? Where do they come from? Why do not they produce eddy-currents in an outside mass of metal, while they were coming in, when being produced? There is a general feeling that they come in from infinity and go out to infinity, and that they ought to have crossed that piece of metal without suddenly starting there. Well, as a matter of fact, when we consider the thing fully from Clerk Maxwell's theory as to the nature of the ether and the way in which it acts, we find that, although according to theories of pure action at a distance there never would be any magnetic force outside the closed-circuit transformer if it was perfectly constructed, according to Clerk Maxwell's theory there would be magnetic force outside a closed circuit which was varying. If you have got a closed circuit in which there is magnetic induction taking place—I think it would be very desirable if more books would call this magnetic induction "magnetic displacement," and would look upon the magnetic force as producing a magnetic displacement. This would be a great improvement, I think, upon the ordinary way of introducing magnetic induction and magnetic force as being

Professor Fitzgerald.

something extraordinary that happens, one in the crevasse and another in the long cylinder. Certainly Clerk Maxwell's view of the subject would be to call one a force and the other a displacement produced by that force. That is quite in accordance with his views, and I think it is the sound way of looking at the whole subject. Well, now, if you look at the magnetic displacement taking place in the iron, while it is taking place in one direction you may talk of it being magnetic current in one direction, and when it comes back of its being a magnetic current in the other direction. We are all familiar with the idea that an electric current has magnetic force in circles outside it. We never think of an electric current at all except as a core to magnetic force. You may as well look at magnetic current as the core of electric force around it. That electric force in the circle round it is the thing that produces the currents in the wires that are round our core. What was Clerk Maxwell's special particular invention in the whole theory? His idea was that magnetic force produced magnetic displacement, and electric force electric displacement; that you do in any medium, including ether, produce magnetic displacement by magnetic force, and electric displacement by electric force; and if you act upon anything that is a good electric medium, like shellac, you produce considerable electric displacement, although, of course, not at all comparable with the magnetic displacement in iron. What is the electric force on the ether due to the magnetic current round that magnetic circuit? It can be calculated from the magnetic current by exactly the same laws as the magnetic force in the centre due to the electric current in a tangent galvanometer. What is the effect of that electric force? To produce an electric displacement? What is that electric displacement? It is, while being produced, to all intents and purposes a little electric current. What is the effect of having an electric current in a piece of wire? It would be that there would be circles of magnetic force round it. The electric force that produces a varying electric displacement in the ether is surrounded by magnetic lines of force; so that, according to Maxwell's theory, there are undoubtedly lines of magnetic force even outside a closed circuit transformer, due to the electric

Professor Fitzgerald.

displacements going on. If we neglect the electric displacements there would not be any.

Therefore I say, in accordance with the old theories, in which there was no electric displacement, there is no magnetic force. Well, now, how much is this magnetic force? what is its intensity? It has an extremely small intensity. The electric force at that place is small in the electro-magnetic measure, and the electric displacement produced is miserably small, because in electro-magnetic measure we have to multiply the electric force by the specific electrical inductive capacity of the medium, which in a magnetic measure is 10 to the minus 21. That is a thousand million of million of millionths; so that the electric displacement is only a thousandth million of million of millionths of the electric force; consequently the magnetic force that exists there would be miserably small. Well, as I said, these magnetic forces that exist outside the closed-circuit transformer, due to the displacement that Maxwell supposes and that Hertz has proved to exist—these are extremely minute in ordinary work. But take into consideration what would happen if we multiply our alternations. You are all familiar with the fact that the electric force is proportional to the number of alternations per second: suppose they ran up to millions per second. At a recent meeting here a dynamo was referred to—I do not know if it was shown—which had a million alternations. What will the electric displacement current then come to? The electric displacement current then will become large—something like a million of million times what it would be with one alternation per second; and consequently if we had ten millions per second, we should be coming to a quantity which would very rapidly wipe out this 10 to the minus 21; and consequently we do find in Leyden-jar discharges, in which you have alternations at the rate of millions per second, these lines of force due to electric displacements which Maxwell supposes to exist: in the case of Leyden-jar discharges these alternations do become of importance. The Hertzian waves can become generated, because you have them. Unless the rate of alternation is sufficiently great, you cannot have any sensible radiation. Therefore, if our alternating dynamos ever come to

the point of alternating at the rate of tens of millions per second, we have induction produced in all circuits in the neighbourhood; but until we do use these very rapid alternations I suppose it is not worth while paying attention to it. Professor Fitzgerald

Mr. R. W. WEEKES: The remarks we have heard from Professor Fitzgerald are very interesting, but on the subject of eddy-currents in the conductors he has overlooked a practical consideration. If the thick wire is wound outside, for a fixed drop of potential you have to use a wire of larger sectional area than when it is wound next to the core. With the assumed distribution of leakage lines this is a point in favour of winding the thick wire next to the core, as the eddy-current loss would be less. Mr. Weekes.

Experiments have been made on a 6-kilowatt transformer at the works of Messrs. Johnson & Phillips, which show that the eddy-current losses in the conductors are inappreciable.

The losses in the same transformer were tested, 1st, when wound complete with the thick wire next to the core; 2nd, when unwound, but energised by a thin wire in place of the thick secondary. The losses in each case were found the same, within experimental errors.

There is one other point I wish to make a few remarks upon, and that is the condenser effect mentioned towards the end of the paper. Early in 1891, when Messrs. Kolkhorst, Thornton, and I were experimenting on the Ferranti effect at the Central Institution, we found that, when switching a large condenser off an alternating circuit, we sometimes got a high P.D. left on the condenser.

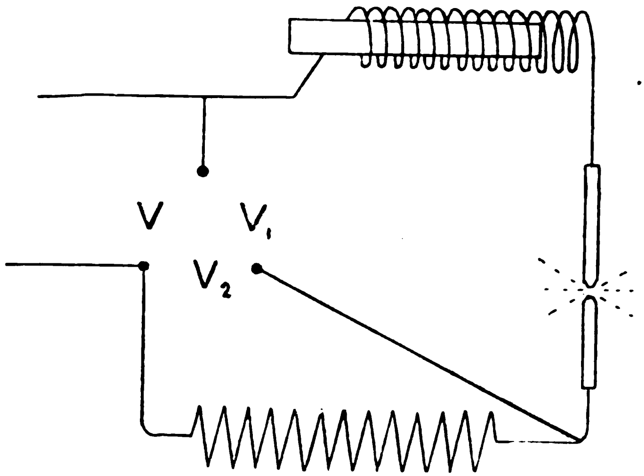
When using 100 volts at a frequency of 140, we got, on switching off, potential differences as high as 570 volts with a 40-microfarad condenser. The consecutive readings were irregular, such as 108, 70, 320, 0, 260, 60, &c.

We experimented extensively as to the causes of this effect, and the following are the conclusions we arrived at:—

Within the limits tried the effect is independent of the capacity of the condenser and the self-induction of the source of current supply. With a quick break switch the voltages left on did not exceed values occurring in the E.M.F. curve of

Mr. Weekes. the alternator. Breaking at known points in this curve gave constant results. Finally, that a slow break switch was necessary to get high rises of P.D., and that any resistance in the circuit reduced the maximum rises. It is interesting to note the similarity of this conclusion to that arrived at by Mr. Mordey—that a slow make is necessary in order to get a rush of current into a transformer when switching on.

The effect being thus due to the arc at the switch led us to experiment on the behaviour of the alternate-current arc. We used a hand-regulated lamp in series with a solenoid with adjustable core, as shown in the accompanying figure, and measured the



angular lag of the current by the much-abused three-voltmeter method. The results varied very much with the nature of the carbons used, as shown in the following table. The core was drawn out by inch steps, and the lag measured in each case. The results given are the means of three readings for each position.

Mr. Weekes.

—	Variation as Core is removed by Steps.	Soft Carbons, Steady Arc. Current, 14·9 amps.; Volts on Arc, 35.			Hard Carbons, Hissing Arc. Current, 14·9 amps.; Volts on Arc, 45·5.		
L.	Position of Core.	V_1.	Angular Lag.		V_1.	Angular Lag.	
			Deg.	Min.		Deg.	Min.
	1	72·5	53	0	...	...	
	2	66·0	51	50	...	...	
	3	58·5	47	0	71·5	55	0
	4	52·6	43	0	64·8	57	20
	5	46·0	35	40	59·0	54	20
	6	41·7	26	10	54·4	54	40
	7	41·4	31	0	53·2	56	0
	8	41·3	30	0	...	...	
	9	40·9	28	50	...	...	
	Core right away }	40·9	27	10	54·8	58	30
	Arc alone	35·5	21	0	45·5	62	40

Analysing these results, it will be seen that the arc always acts as a self-induction, but that the hissing arc has about three and a half times the self-induction of the steady arc. Thus, in the cases with the arc above, the steady arc requires 12·5 volts to overcome the self-induction, and 33 volts to overcome the resistance and back E.M.F., assuming the two to be at right angles. In the hissing arc, with 62° lag, the self-induction voltage rises to 40, and the voltage required to overcome the resistance and back E.M.F. is only 21·5.

We succeeded afterwards, by balancing this self-induction by capacity, in running a clear hissing arc between hard carbon with only 22 volts across the terminals, with 16 amperes passing.

Prof. J. Perry: I have been asked by the President to say something about the voltage drop in transformers—that part of the drop which is due to magnetic leakage. Until the President sent me a note to the effect that he thought the members of this Institution would like to hear a short explanation of how it is Professor Perry.

Professor Perry.

that the drop occurs, I thought that every member of the Institution was already aware of the reason. The President, however, assures me that it would be well to try to give a simple explanation. I have already published, in several papers read before the Physical and Royal Societies, what I thought was a simple enough explanation, but it is quite possible that those papers may not have been read by many members of this Institution. I felt when the note came that I could give a simple explanation of the phenomenon to the members, but I have changed my mind, and know now that it is not quite so easy to give one to persons who say they scorn, and who certainly dislike, mathematics. So I am told not to make it mathematical! When one tries to explain anything about a transformer to oneself, one has one's own analogy—one's own way of looking at the phenomena of transformers. If one tries to explain it to somebody else, he does not understand it, because he has his own private little analogy. Hence the only general, and, indeed, the only correct, way to explain a transformer is by means of mathematics. I almost wish now that the President had not asked me to explain this thing. I have proved in my Royal Society paper of about a year ago that in all calculations on transformers except one—namely, where you want to calculate the current supplied to an unloaded transformer—it makes no difference whether you take into account hysteresis, eddy-currents, and so on, or assume that they do not exist. I mean in calculating drop of potential, and that sort of thing. It is of no consequence what law of magnetisation you assume; it comes to the same, practically, as if you assume the magnetic permeability of the iron to be constant. The result of the calculation is known to everybody, I hope.*

---

* The equations to the two circuits may be written in various ways. Taking the method given by me in the *Philosophical Magazine*, August, 1891, p. 182, but altering the lettering,

$$V_1 = (R_1 + L_1\,\theta)\,C_1 + M\,\theta\,C_2\,;$$
$$O = M\,\theta\,C_1 + (R_2 + L_2\,\theta)\,C_2,$$

where R_1 and R_2 are the resistances, and L_1 and L_2 are the self-inductions of the two circuits, and M is their mutual induction. Solving, like ordinary simultaneous equations, in algebra, we find,

$$-C_2 = M\,\theta\,V_2 \div \{R_1\,R_2 + (R_1\,L_2 + R_2\,L_1)\,\theta + (L_1\,L_2 - M^2)\,\theta^2\}.$$

Unless the frequency is very small, in an ordinary transformer, even when R_2 is very large, $R_1\,R_2$ is negligible, and hence we have the result (stated at page 183,

Philosophical Magazine, August, 1891): "The secondary current is the same as "if the primary voltage acted in one circuit of resistance, Professor Perry.

$$\text{“}R_1 \frac{N_2}{N_1} + R_2 \frac{N_1}{N_2},$$

"and self-induction, $(L_1 L_2 - M^2) \div M$, there being no other circuit." This self-induction is evidently $M x$, as stated above, where $M = N_1 N_2 4 \pi a \mu \div 10^9 \lambda$, where a = cross section of iron in sq. cm., μ =, say, 2,000; λ = average length of the iron circuit.

Evidently, if $R_2 = r_2 + \rho$, the part r_2 being internal, then for practical cases—

Secondary voltage, if there is no load, $= \frac{N_2}{N_1} V_1$;

Fractional drop for any load, no magnetic leakage, $= (R_1 \left(\frac{N_2}{N_1}\right)^2 + r_2) \div \rho$;

Extra fractional drop due to magnetic leakage, when small, $= 2 (\pi f N_2 M x \div \rho N_1)^2$, and a lag in degrees $360 M x f N_2 \div \rho N_1$.

It is obvious that any change due to transforming up instead of transforming down can only be due to the value taken for ρ—that is, what is meant by similar loads in the two cases. The drop depends upon $\frac{N_2}{N_1 \rho}$, and at full load this ought to be much the same whether we are transforming up or down.

Some readers of this note may prefer the other method of treatment—see my paper, Royal Society, March, 1892, where I take,

$$V_1 = R_1 C_1 + N_1 \theta I;$$
$$O = R_2 C_2 + N_2 \theta I,$$

where I is the induction.

This leads to the result given at page 459—that, whether there are eddy-currents or hysteresis or not, we have always the practically true rule,

$$C_2 = V_1 \div \left(\frac{N_1}{N_2} R_2 + \frac{N_2}{N_1} R_1\right).$$

If, however, there are condensers or choking coils attached to the primary or secondary circuits, R_2 and R_1 are not mere ohmic resistances. Thus, if $\frac{1}{p}$th of the induction produced by the primary escapes the secondary, and if $\frac{1}{q}$th of the induction produced by the secondary escapes the primary, we must use $R_1 + l_1 \theta$ and $R_2 + l_2 \theta$ instead of R_1 and R_2 merely, where $l_1 = \frac{1}{p} N_1^2 \sigma$, and $l_2 = \frac{1}{q} N_2^2 \sigma$, if $\sigma = 4 \pi a \mu \div 10^9 \lambda$. Of course, what I call M is $N_1 N_2 \sigma$. At page 460 I give an interesting table showing the drop in the induction, I, and its increasing lag due to this magnetic leakage.

In the Mordey transformer, to which I refer above, $R_1 = 27$ ohms, $N_1 = 460$ turns, $r_2 = 0{\cdot}067$, $N_2 = 24$ turns, $a = 360$ sq. cm., $\lambda = 31$ cm., $\mu = 2{,}000$, $M = 3{\cdot}22$ secohms; ρ, the resistance of secondary outside the transformer, is taken as 10 ohms at full load.

The secondary current is as if we only had one circuit in which there is a voltage V_1, resistance $2{\cdot}7 + 19{\cdot}17 \rho$, and self-induction $3{\cdot}22 x$. The foregoing rules become—

Secondary voltage, if no load	$V_1 \div 19{\cdot}17$;
Fractional drop at full load, if no magnetic leakage	$= \frac{2{\cdot}7}{19{\cdot}17 \rho} = 0{\cdot}0141$;
Extra fractional drop due to magnetic leakage at full load	$0{\cdot}005567 x^2 f^2$;
Lag due to leakage at full load	$6{\cdot}047 x f$ degrees.

Professor Perry.

The simple formula is this: If V_1 is the primary voltage, and N_2 is the number of turns on the secondary, N_1 the number of turns on the primary, and R_1 is the internal resistance of the primary, and R_2 is the whole resistance of the secondary, then the secondary current is just the same as if you only had one circuit, and in that circuit the primary voltage, V_1, acted, and the resistance of that circuit—an imaginary resistance—was,

$$\frac{N_2}{N_1} R_1 + \frac{N_1}{N_2} R_2.$$

That is a simple rule, which I believe everybody uses, although he may not understand it when written down mathematically.

Now, how does magnetic leakage come in? I have given the result, if you assume no condensers, no choking coils, and no magnetic leakage. What do you mean by magnetic leakage? It may be defined in quite a lot of ways, but this is a simple and sufficiently accurate way of putting it. The current in the primary coil produces some induction which does not thread through the secondary coil; the current in the secondary coil produces some induction that does not thread through the primary coil. The fractional leakage in each case may be taken. Suppose the sum of those two—you may take them as being about equal—the sum of those two I call x in the following statement.

If you only think a little, you will see that if some of the induction of the primary leaks, it is as if we had a little choking coil in the primary. A similar statement may be made about the secondary. The effect, then, of leakage is to cause the R_1 and R_2, already mentioned, to be not merely ohmic resistances, but to have self-induction. The true rule, then, is: *The secondary current is just the same as if you only had one circuit, and in that circuit the primary voltage,* V_1, *acted, and that circuit had a resistance,*

$$\frac{N_2}{N_1} R_1 + \frac{N_1}{N_2} R_2,$$

and a self-induction $M x$. Here M is the mutual induction of the two coils. Divide this $M x$ by the imaginary resistance and we have an imaginary *time constant*, which I will call s. The effect of a time constant s, when small, is to diminish the current 200

$8^2 \pi^2 f^2$ per cent., if f is the frequency, and to produce a lag of 360 s f degrees. Evidently the drop due to leakage is proportional to the square of the load. Professor Perry.

Thus, in a 1,500-watt Mordey transformer, where M = 3·22, and the imaginary resistance works out to be 2·7 + 19·17 ρ, ρ being the external resistance of secondary, and s is practically 3·22 x ÷ 19·17 ρ; the drop, if there is no magnetic leakage, is 1·41 per cent. if ρ = 10 ohms at full load. If we take it that 1-200th of the induction of each of the coils leaks past or escapes the other, then x = 0·01, and we have the following results:—

f, Frequency.	Lag due to Magnetic Leakage at Full Load.	Increase of Drop due to Magnetic Leakage at Full Load.	Total Drop at Full Load.
	Deg.	Per cent.	Per cent.
100	6	0·56	1·97
150	9	1·25	2·66
200	12	2·23	3·64
250	15	3·48	4·89
300	18	5·01	6·42

I see no reason why there should be much difference in the percentage drop whether one is transforming up or down at full loads. Any difference will probably be accounted for by one's not knowing what one means by equal loads in the two cases. Observe that diminishing x or diminishing M will diminish the drop; and if this were the most important fault in a transformer, it would be easy to arrange the sizes and resistances so as to make the drop at full load smaller and smaller.

Professor Ayrton asks me to refer to the experimental results which he has placed upon the wall. I believe that, although I predicted the result two years ago, and gave the law of it, and gave in tables numerical results such as were to be expected in practice, Professor Ayrton was the first who measured the drop,

Professor Perry.

and he published his results last summer at the British Association meeting.*

Professor Ayrton's Results.

Transforming *down*, 2,000 to 100 volts—that is, from a high to a low P.D. % drop at *full* load.

Frequency in Periods per Second.	Mordey Closed-Circuit Transformer.	Swinburne Open-Circuit Transformer.
100	1·72	2·38
150	2·1	3·3
200	2·63	4·1

Transforming *up*, 100 to 2,000 volts—that is, from a low to a high P.D. % drop at *full* load.

Frequency in Periods per Second.	Mordey Closed-Circuit Transformer.	Swinburne Open-Circuit Transformer.
100	2·06	2·5
150	3·00	3·0
200	3·15	4·0

If I had details of the sizes and resistances and numbers of turns of these two transformers, I have no doubt whatever that I could exactly reproduce these numbers by calculation. Some of you might have thought that, as percentage leakage is greater in the "hedgehog" (that is, x is greater), there ought to be a greater drop. Now this drop depends upon M as well as x, and also on what is meant by "full load." Considering M x: although x is greater in the "hedgehog," M is smaller; and so,

* Although I published the theory in 1891, and gave tables of drop due to magnetic leakages at different frequencies and loads (pp. 178–180, *Phil. Mag.*, August, 1891; see also a paper read before the Physical Society, May, 1891), I see that (p. 184), although I referred to a confirmation of my predictions by actual experimental results on transformers, it is evident that such confirmation can only have been very rough when compared with the confirmation as to frequency given now by Professor Ayrton, and as to load by Dr. Fleming.

on the whole, one is not surprised that the drop in the "hedgehog" is not much greater than in the Mordey. Professor Perry.

To explain *drop* due to magnetic leakage, we have simply to remember that, if any of the induction in either coil escapes the other, it is as if we had no leakage, but had a little choking coil in which the induction is the leaked induction.

Mr. H. C. Haycraft: There is one point of considerable interest towards the end of Dr. Fleming's paper to which Mr. Mordey has already referred, namely, that of current-rushes and rises of pressure in transformers at the moment of switching on. Some experiments were made at the Central Institution last year which may be of interest, though not bearing exactly on the subject, as a condenser was used, and it is possible that the effect Mr. Weekes has just described occurred. Fig. A illustrates our Mr. Haycraft.

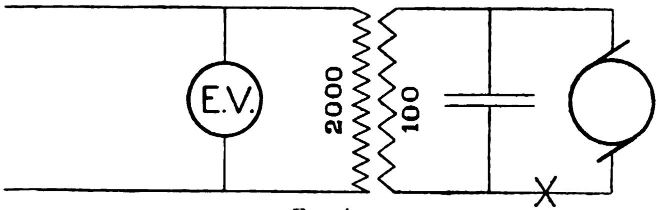


Fig. A.

arrangement. We were using a Ferranti alternator, the frequency being rather high—188—and a condenser was supplying the magnetising current to a "hedgehog" transformer, the main current being about 3 amperes, and the primary magnetising current $9\frac{1}{2}$ amperes. We were transforming up from 100 to 2,000 volts. The secondary was open, and an electrostatic voltmeter was placed across it. The condenser circuit was broken accidentally by moving one of the wires from the condenser terminals, the wire being immediately afterwards replaced. We then found that the voltmeter had ceased to read, and we found, on examining it, that the fuses had gone. The voltmeter had been tested up to 3,000 volts alternating, and it was obvious that an abnormal rise of potential had occurred. The capacity we were first using (64 microfarads) was very nearly that most suitable for that particular transformer at that frequency; the same effect afterwards occurred with a smaller capacity (40 microfarads). In

Mr. Haycraft. neither case was anyone watching the voltmeter, so I cannot say whether the rise took place on breaking or making the condenser circuit. We were not looking for high potentials at the time, and made no further investigations.

The next case was more like that Dr. Fleming refers to. Some experiments were being made by Mr. Mather at the Central Institution. He was transforming up from 50 to 2,000 volts, using a Mordey transformer. The current was supplied at very low frequency by a Gramme machine used as an alternator, two opposite points on the armature being connected to rings on the shaft from which the current was taken. The Gramme was running as a motor from 50 cells, the fields being separately excited. On two occasions the fuses of an electrostatic voltmeter on the secondary of the transformer went, showing that a potential of above 3,000 volts alternating had occurred. The transformer secondary was open at the time. Examination showed that the brushes of the Gramme were loose, and that the effect could be reproduced by lifting them, or by breaking the driving circuit at the cells. On one occasion the rise was seen on the voltmeter, the needle only rising a little above 2,000, and then falling to zero.

I made some further experiments with Messrs. Woodhouse, Baylay, and Clift. In these tests—illustrated in Fig. B—the

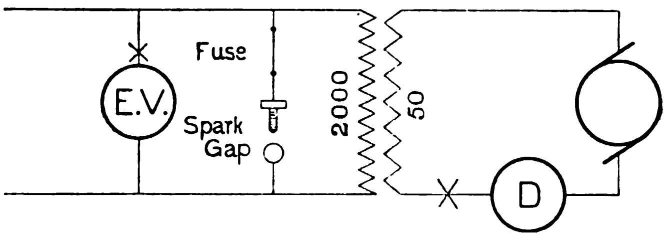


Fig. B.

secondary circuit of the transformer was open, an electrostatic voltmeter, calibrated up to 3,000 volts alternating, being across the terminals. As the voltmeter fuses had gone several times in the former experiments, it was decided to protect them by using a spark gap instead of the voltmeter, and calibrating it by aid of the voltmeter before testing. One disadvantage of

Mr. Haycraft.

this is that it is impossible to know whether a rise in the secondary potential coincides with a current-rush in the primary. A set-up dynamometer was used to detect the current-rushes in the latter; they were very frequent, and sometimes very violent, the dynamometer being set up to two or three times the normal current. The frequency in these experiments was 25, the Gramme alternator being used, as before. During some preliminary experiments the electrostatic voltmeter was left on the secondary, and at the tenth make of the primary circuit the fuses went, indicating a potential exceeding 3,000 volts alternating. The Gramme alternator was not very suitable for these tests, as the volts rose from 50 to 60 when the transformer primary circuit was broken, the volts being thus 60 at the instant of switching on to the transformer; but this will not explain the large secondary rise. I may mention that this Gramme current was not like the ordinary alternating current; at any rate, the E.M.F. curve is not a sine wave. The ratio of the maximum of the E.M.F. to the square root of the mean square was taken, the maximum being simply the volts on the brushes of the Gramme, and the square root of the mean square being given by a Cardew voltmeter on the mains. The ratio was 1·95 to 1, instead of 1·4 to 1, when the transformer magnetising current was flowing.

We next substituted a Ferranti alternator for the Gramme and repeated the experiments. The frequency was 144; the volts were made 50 on open circuit, and dropped to 47 on closing the switch. The volts on the mains therefore never exceeded 50, which corresponds to 2,000 in the secondary. The spark gap was first adjusted to 2,270 volts: loud sparks passed at the 3rd and 78th makes. A Cardew voltmeter across the mains showed no effect whatever. The secondary volts while running steadily were only 1,850. The gap was then tested, and did not spark at 2,280 volts alternating, so there was certainly a rise of about 300 volts. In another case the gap was adjusted so that it did not spark at 2,100, yet several sparks occurred in 50 makes; the gap being afterwards tested up to 2,230 volts without sparking. In the last of my experiments at this frequency the gap was increased until

Mr. Haycraft.

it failed to spark at 2,350 volts alternating. At the 166th make a spark passed, and then 600 makes produced no effect. The gap afterwards broke down at between 2,300 and 2,400 volts alternating; it was 0·76 mm. long. At this frequency—144—there were practically no current-rushes, except, of course, when sparks passed.

In the first experiments the primary circuit was closed by a switch, but afterwards, to save the exertion of making many hundred contacts with the switch, the circuit was closed by a wire which was tapped on one of the rubbing contacts of the switch. Testing again with current from the Ferranti at a frequency of 100, I found the same rises of secondary pressure, the spark gap going several times after it had been adjusted to stand 2,000 volts alternating without sparking. The current-rushes were again very few; there was one very distinct rush without a spark occurring, but only one.

In the last experiments I tested for rise of pressure on the mains. An electrostatic voltmeter reading up to 130 volts was placed directly on the primary terminals of the transformer—that is to say, inside the switch. The needle was prevented by a stop from going below 52 volts: then I expected that if any rise of potential occurred this voltmeter would detect it. We found no such effect, although sparks passed at the secondary gap, and there was one large current-rush *without* a spark.

Our general conclusions are, that current-rushes are very frequent at low frequencies, but can hardly be detected at 100 and above. Also, that the effect *does* penetrate to the secondary—a result which Mr. Mordey's experiments with the lamp on the secondary failed to show; and that the secondary rise of potential may be considerable, even when high frequencies are used. So far as our experiments went, we found no rise of pressure on the primary mains. In all cases we were transforming in the opposite direction to Dr. Fleming and Mr. Mordey, and (except in the cases first mentioned, where a condenser was used) were using one of Mr. Mordey's closed-circuit transformers.

Mr. Sparks.

Mr. C. P. SPARKS: With reference to the rush of current into transformers when switching them on to a live circuit, this was first noticed, to my knowledge, in 1886, but no measurement of

Mr. Sparks.

intensity of the rush was made until that made by Dr. Fleming in 1891. Mr. Mordey was pointing out at the last meeting that the rush was most likely owing to the defect of some special type of transformer. Now the rush is most noticeable in a transformer in which the iron is worked at a high induction. If you take an old-type transformer, it is pretty nearly certain that the induction is very much higher than the modern practice. The 5-H.P. transformer mentioned by Dr. Fleming takes more energy to magnetise it than the 10-H.P. of the same date, viz., 1885. The induction in this transformer at the periodicity it was designed for—viz., 100 ∿ per second—is very high indeed, and in this transformer the rush is very marked. I find that, taking the following Ferranti transformers—the 5- and 10-H.P. of 1885 type, and 15-H.P. 1891 type—and allowing the transformers to be subjected to a greater induction than they were designed for, by using a lower periodicity, it is very easy to see this rush of current. In making some thousand experiments, in each case, the percentage of rushes was as follows:—With 64 ∿ per second the 5-H.P. showed 25 per cent. of slight rushes and 6 per cent. very fierce rushes; with the 10-H.P. there were 15 per cent. of slight rushes and 4 per cent. of fierce rushes; with the 15-H.P. there were 9 per cent. slight and 2 per cent. fierce rushes. With 83 ∿ per second, with the 5-H.P. the slight rushes were reduced to 10 per cent. and the fierce to 2 per cent.; 10-H.P. goes down to 1·4 per cent. and 0 per cent.; in the 15-H.P. nothing at all is observable. With 100 ∿ per second the 5-H.P. goes down to 2 per cent. slight rushes and no fierce ones; in the 10-H.P. nothing occurs; and in the 15-H.P. nothing at all. Now, the higher the induction the greater the residual magnetism would be, and therefore the more intense the rush of current. A bad quality of iron would also give the same result. I think Dr. Fleming's explanation of the cause of the rush is the right one. Well, then, I think it was in 1891 there was considerable discussion here as to the merits of closed and open circuit types of transformers, and in order to set it at rest Dr. Fleming proposed to measure the true watts wasted in each case by several methods, and to do this Mr. Swinburne's very excellent wattmeter method and the three-voltmeter and

Mr. Sparks three-ampere methods were proposed; and also at the same time Mr. Wright requested him to try a method he had had in use at Charterhouse Square about a year previously, namely, the split-dynamometer method. This method of testing true watts was a modification of some of Mr. Blakesley's work—an extremely simple modification—and I think Mr. Wright deserves every credit for it. This has been shown by Dr. Fleming to be not only the simplest, but the best. Now we have made since 1890 tests by what I call the split-dynamometer method, and our tests of the transformers agree so completely with Dr. Fleming's that there is no doubt that ordinary electrical people (the layers-on of the paint, and not the grinders of the colours) will see that extremely practical results can be obtained with care and selection of the instrument. We use in series with one coil a number of lamps, in order to have a non-inductive resistance, and one additional ampere-meter and voltmeter. Here you have at hand everywhere a method of measuring the true watts which is extremely simple and very practical.

The CHAIRMAN: By the split dynamometer, if I understand rightly, you mean a wattmeter with a low-resistance pressure coil?

Mr. SPARKS: Well, our split dynamometer is an ordinary Siemens dynamometer, bought eight or nine years ago; there is no special arrangement. We measure the true watts in transformers in the following way:—Not having facilities for measuring the watts on the high-pressure side, we always take the efficiency of two transformers together. We measure the watts on the low-pressure side, connect the high-pressure sides together, and take energy from the secondary of the second transformer. Then we take the efficiency, and divide the losses on the two transformers. Of course I am only speaking now of workshop tests. Dr. Fleming has shown how difficult it is to get results of great accuracy, but we get within 3 or 4 per cent. of the mean of some hundreds of tests he has taken, and I think that is good enough for everyday work.

Mr. Wright Mr. ARTHUR WRIGHT: This subject has interested me very much, as I have been working some time in this field. I must congratulate Dr. Fleming on the excellent care and amount of

labour he has bestowed on this intricate and tiresome subject. Mr. Wright
The amount of work involved in the preparation of his paper can only be known to those who have tried to do similar work. Another valuable feature might be mentioned of this paper: it has been written without much introduction of mathematics, and shows how much useful work can be done in alternating-current studies without any introduction of self-induction coefficients or the sine-curve mathematics of the old school, and by the introduction in their stead of the ideas of permeability and of the mean induction, which I believe is all that is necessary for practical purposes. A coefficient of self-induction ceases to be an intelligible term when the ideas of the sine curve, or constant permeability, disappear. Everybody now knows the sine-curve form of current in an alternating transformer at little or no load does not exist. As I designed the 1892 Ferranti transformers referred to in the paper, I am very glad to find that the loss came out very nearly as calculated. The 20-H.P. transformer was designed to have 1·4 per cent. iron loss; I see Dr. Fleming found it as low as 1·3. I think it is rather unfortunate that Dr. Fleming should have taken the 1885 5-H.P. size of Ferranti transformer, as it was the worst transformer ever built, and, as Mr. Sparks has said, the 10- and 15-H.P. absorbed less magnetising watts than the 5-H.P. A great number of the latter were taken off the L.E.S.C. circuits over two years ago, and it cannot be said to represent in any sense the state of transformer science in 1885. I think, if he had taken the 10-H.P. and the 15-H.P. and made as complete tests as he has done with the 5-H.P., he would have marvelled at Mr. Ferranti's great skill in designing in those days, when nothing was known about waste in iron and magnetic leakage. The great improvements in modern transformers are apparently entirely due to the adding of more copper to reduce the induction in the iron. An 1892 20-H.P. has something like four times as much copper as the 1885 20-H.P.; that is equivalent to saying, I think, that the old 20-H.P. transformer was only really a 5-H.P. transformer. I notice Dr. Fleming has drawn attention to a small transformer made by Messrs. Johnson & Phillips having very good regulation; and I do not understand why special emphasis was laid on the good

Mr. Wright. regulation of a small transformer. I found it was just as easy to obtain good regulation in a small transformer as in a large one, even without sandwiching, by having long coils, or, rather, the section of the coil space very long in comparison with its depth. Dr. Fleming states that the centre of the "hedgehog" core has an induction of 10,000: I suppose that means the maximum induction. I should like some information on this point, as it seems abnormally high. He also mentions that about 30 cubic centimetres of core in most transformers wastes 1 watt in hysteresis; whereas, from my experience, I find this figure comes out about 120 or 110 cubic centimetres. I suppose he was referring to the old 1885 type, with their high induction. Dr. Fleming hopes that very soon we shall be able to get 90 per cent. at one-tenth load. Surely this is only a question of money. The way to design transformers has been known a very long time, but it is the excessive cost of these high-efficiency transformers which limits the efficiency attainable in practice. To diminish the 1½ to 1 per cent. loss would almost double the cost of the transformer. Notwithstanding the high efficiency now obtainable, a 20-H.P. transformer, with its feeder, costs in upkeep per annum, including interest, the same as a 20-H.P. low-tension feeder a mile and a quarter long; it therefore seems evident to me that, even with this 98½ efficiency, there is no room inside a radius of a mile and a quarter for permanently connected transformers, owing to the continual iron loss. I am often puzzled to predict what will become of the companies where these permanently connected transformers are used, and I think this paper of Dr. Fleming's ought to bring the matter more prominently before engineers, and demonstrate that it is not so commercial to insist on higher efficiency transformers as to disconnect cheaper ones when not required. I have been fortunate enough to supply the same town on the two rival systems, and I know that while it used to take nearly 25 lbs. of coal per unit sold on the high-tension system, it now takes on an extremely low-tension system only 9 lbs., working in the same area and supplying about the same number of units. I think that fact must show alternate-current engineers that something ought to be done in the way of cutting out transformers,

or in diminishing the loss of iron at no load in some way such as Mr. Ferranti suggested in 1885. Mr Wright.

Professor A. B. W. KENNEDY [*communicated*]: The Institution is to be heartily congratulated upon receiving a paper of so much value as Professor Fleming's. This paper furnishes—practically for the first time, so far as my knowledge goes—the materials necessary for a really thorough examination of the efficiency of transformers as used in electric lighting. Professor Kennedy.

Two points seem to call, in the first place, for special recognition. The first is the most remarkable and satisfactory improvement which has been apparently made in the construction of transformers within the last few years. The engineers who have been devoting their attention to this matter, and who have produced the apparatus whose efficiencies are shown in most of Professor Fleming's curves, have achieved a most remarkable success, and a success most important in connection with the distribution of electricity.

The second most noteworthy point appears to me to be the close parallelism between the lines representing the copper and the iron losses in a transformer. In nearly all the cases given by Professor Fleming it appears that the actual total losses could have been fairly approximately determined by a single experiment of the losses on open circuit. The copper losses can of course always be calculated for any load, and by adding the losses so calculated to the iron loss at no load a very fair approximation of the total losses could have been obtained.

It goes without saying that from the point of view of those of us engaged in practical electric lighting the all-day losses are those which have to be considered, and Professor Fleming's paper is so clear and ample in its information that there is little difficulty in deducing these from the figures he gives, combined with the knowledge which we already possess of the habits of those who use electric light. From the examination of a number of cases, I find that in London, at any rate, one is not far wrong in assuming that the average householder uses energy sufficient to keep all his lamps alight for one hour a day all the year round. For shops (closing at London hours) the consumption is about

Professor Kennedy.

one and a half hours per day, and for clubs about three hours per day. I have worked out the all-day efficiency of transformers for each of these three cases, and, in order to get the figures in convenient form, will assume that in each case the maximum requirement of the installation is 1,000 watts.

Taking a private house which would require 1,000 watts if all its lamps were alight simultaneously, it might probably be sufficient to put in a transformer whose maximum capacity was 750 watts only. The energy used would be one lamp-hour per day for every lamp installed. The lighting hours would average about six out of the 24, and the hours when the secondary circuit was open would be about 18 hours out of the 24 (the exact subdivision of the hours does not, as can easily be seen, make much difference in the final result). The average number of lamps alight at one time during the six hours when any lighting at all is on is one-sixth of the maximum installed. The actual work done, therefore, during the 24 hours will be $\frac{1,000}{6} \times 6 = 1,000$ watt-hours. The efficiency of the transformer at the supposed load will be about 88 per cent. The waste work during the hours of lighting will therefore be 136 watt-hours. If the magnetising energy be $2\frac{1}{2}$ per cent. of the full load of the transformer, the loss during the 18 hours of no load will be 338 watt-hours. The total losses will thus amount to 474 watt-hours. The efficiency of the transformer will therefore be $\frac{1,000}{1,474}$, or 67·8 per cent.

In a shop the average hours of lighting may be taken as three, and the average hours with no light as 21. (This is remembering that for many months of the year a shop uses practically no light whatever). Under these conditions the useful work per day will be 1,500 watt-hours. The waste work during the hours of lighting, the transformer efficiency being taken at 93 per cent., is only 113 watt-hours. The waste work due to losses while the secondary circuit is open is 525 watt-hours. The total losses therefore amount to 638 watt-hours, the useful work being 1,500. The efficiency under these circumstances is

Professor Kennedy.

70 per cent. I have assumed here that the transformer is one of 1000-watt capacity, as it would not be safe for a shop to use a transformer smaller than would carry all its lights on at once.

In the the case of a club—assuming also that the transformer is for 1,000 watts—the hours of lighting may be taken as averaging seven out of the 24. The actual work per day will be, at the rate given above, 3,000 watt-hours. The transformer efficiency will be about 92 per cent., and the losses during lighting hours will be 261 watt-hours. The losses during non-lighting hours will be 425 watt-hours. The total losses come, therefore, to 686 watt-hours, and the average all-day efficiency to 81·4 per cent.

With a transformer, therefore, of the very highest efficiency, and using 2½ per cent. of its maximum energy continuously, on open circuit there will be an average loss of (say, in round figures) 32 per cent. for a private house, 30 per cent. for a shop, and 18 per cent. for a club, assuming the hours of lighting and the consumption of current to be those which actually exist in those districts of London with which I am best acquainted.

If the transformer could be made, as larger transformers no doubt can be made, to waste a less percentage of energy in magnetising current, the efficiencies would be correspondingly raised. But, on the other hand, it has to be remembered that the magnetising loss of only 2·5 per cent. corresponds to a very much larger transformer than that used by the average consumer. The size of transformer which I should mostly have to use in London would be from 1,500 to 2,000 watts—much smaller sizes than those tested by Dr. Fleming. These transformers have a magnetising loss of about 4 per cent., and under these circumstances their efficiencies would fall to 59, 60, and 75 per cent. respectively. It appears obvious from these figures that the all-day efficiencies of even the very best transformer fall terribly short of the actual efficiency of the same apparatus, even working at quite low loads. It is clear also that the greatest part of the losses occurs during the hours when the secondary circuit is open. In the case of the transformer which I have supposed, wasting 2½ per cent. of its maximum out-

Professor Kennedy.

put in iron losses, the waste during non-working hours is, roughly, three times as great as that during the real hours of lighting. The absolute necessity is thus emphasised of reducing the iron losses to the very smallest possible extent. I have worked out the all-day efficiencies for transformers in which these losses amounted to some of the larger quantities given by Dr. Fleming, but refrain from putting them on paper!

By the use of the system which is being adopted by the City of London Company, and elsewhere, of banking the transformers and supplying consumers by low-pressure mains, the heaviest part of the transformer losses are of course avoided, as no transformer would ever be left with open secondary, or would be allowed to work at a very low efficiency. This system, however, is only applicable in cases where the cost of low-tension mains is not prohibitive, and so in most cases where it can be used there are alternative possibilities. I feel, therefore, that more importance and interest really attaches to the case where transformers are used in the consumers' houses in sparsely built districts, and where practically electrical distribution can only take place at all with the use of separate transformers. In this case the only apparent way of increasing the terribly low all-day efficiency in private houses is by arranging that the transformer shall be entirely cut out of circuit when the lights are turned out in the house. In certain cases which have come under my observation there has been no difficulty in doing this in shops, where the person last to leave the shop cuts out the transformer with an ordinary switch. In the case of a private house it does not seem yet to have been found possible to arrange practically that the actual extinguishment of the last light should cut out the transformer automatically, and the actual putting in of the first light should put in the transformer automatically. If some arrangement of this kind cannot be devised (but I hope it can), it would appear necessary that there should be an arrangement (which could probably be worked from more than one point in the house, and particularly from one of the bedrooms) by which the same object could be effected. I should not despair of inducing householders to use such a switch if it were placed quite conveniently to their hands.

It is obvious that in the case of companies supplying current to sparsely populated districts it would amply pay to have even a somewhat expensive arrangement of this kind (which might obviously require the use of a cell or two in connection with a relay), if they could only make reasonably sure that it would be generally used. At present, at all events, it appears certain that some such an arrangement is essential to attain economy of distribution on such a circuit. Professor Kennedy.

One other point may be mentioned as affecting the all-day efficiency of transformers. I have assumed that a transformer was used whose maximum capacity was exactly equal to the required maximum current in the house. As, however, the maximum house currents will vary indefinitely, and there are only a certain small number of sizes of transformers, in three cases out of four the transformer must be larger than I have assumed. So far as efficiency during lighting hours is concerned, the great excellence of the existing transformers makes this a matter of small moment. But the losses during non-lighting hours practically appear to be proportional to the size of the transformer, and therefore the inevitable use of too large transformers must cause very great waste.

In the case of the private house which I have supposed, for instance, if a transformer of 1,000 watts were used instead of the transformer of 750 watts which I assumed, the all-day efficiency would be reduced from 68 to 61 per cent.

I feel bound to put on record that I think a very serious error of judgment has been made by those who have insisted that the loss due to magnetising current might be neglected if only the transformers had a high efficiency all round. No doubt the high efficiency is most important, and the extent to which it has been attained is most encouraging; but in cases where separate transformers have to be used we could well sacrifice something in the efficiency, even at low loads, if we could diminish also the frightful waste occurring with open secondary. From two-thirds to five-sixths of the whole losses will be seen, from the figures I have given, to be due to this cause, even taking the efficiency corresponding to a 6,000-watt transformer. With

Professor Kennedy

the more usual sizes for average consumers this proportion will be more or less doubled.

I have worked out the effect on the all-day load curves of a station, of the very low efficiencies which I have mentioned. I need hardly point out to engineers practically conversant with such matters that the result, although bad enough, is not nearly as bad as it sounds. The peak of the diagram is the part *least* affected, of course, as there the efficiency is at its highest. During the lower day loads, however, the work is often more than doubled. The total plant necessary to keep a certain maximum number of lights going at once is not very much greater (say 10 per cent.) than in a direct-current station with ordinary losses in the mains. The load-factor during the day is greatly raised; but instead of even the St. Pancras 3d. per unit being received for the additional day work, it has to be done (without differential meters) for nothing! I think we should not be far wrong in roughly estimating the net cost of each useless unit at about half the cost of each useful one.

I repeat that I have dealt only with the case of isolated transformers. I have done so partly because this is at present by far the commonest case, but mainly because there are many places in which it is the only possible case. The banked transformer system, on the other hand, can only be used in places where it pays to put down expensive low-tension distributing mains, and where, therefore, although it may in certain cases be the best system, it can hardly ever be the only possible system. As to banking transformers, I would only caution engineers not to be too sanguine as to the high average load that will be so obtained. I have painful experience of the great difficulty of getting a high load-factor for engines, even in the very simple case of direct-current machines running in parallel. With transformers it will be, for sufficiently obvious reasons, very much more difficult. Dr. Fleming's efficiency curves, however, show the maximum efficiency to cover such an enormous range of output that probably even a 50 per cent. load-factor would not much affect the economy of working—a condition of affairs which is, of course, much more favourable than in the case of engines.

The CHAIRMAN, in moving a vote of thanks to Professor Fleming for his valuable paper, stated that in consequence of that gentleman's unavoidable absence his reply to the several speakers would be deferred until a future meeting.

The CHAIRMAN announced that as the result of the ballot the following candidates were elected:—

Member:

Harold Sherwin Holt.

Associates:

James Bell.
Prof. W. H. Bragg, M.A. Cantab.
Joseph Bevan Braithwaite, jun.
William Frederick Burton.
Alexander Cook.
Edwin Philpot Crowther.
Thomas Devitt.
Reginald Sydney Downe.
Robert James Elmhirst.
Clinton Coleridge Farr, B.Sc.
Robert Gibson.
Samuel Henry Gowdy.
Charles Henry Hill.
Edwin Human.
Robert Reid.
Frederick William Reynolds.
John B. Saunders, jun.
Edward Carstensen de Segundo.
F. C. Sinclair.
James B. Smith.
Samuel Spencer, Assoc. Inst.C.E.
Robert Sykes.

Students:

Joseph H. D. Brearley.
A. C. Cormack.
Robert Arthur Fullerton.
Frank Hewer.
Henry Cook Leake.
Joseph McMahon.
Christopher Mark Mayson.
Arthur Cecil Herbert Orpen.
C. G. Paget.
G. F. Pilditch.
Chas. Henry Prichard.
Albert Edward Robinson.
Hugh Innes Rogers.
Philip James Watts.

The meeting then adjourned.

The Two Hundred and Forty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 26th, 1893—Professor W. E. AYRTON, F.R.S., late President, in the Chair.

The minutes of the Ordinary General Meeting held on January 12th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

J. R. Blaikie.	Wilfred M. Oliver.
Francis L. Lister.	F. S. Paterson.
R. J. F. Mostyn.	J. C. Shields.
J. Allison Muir.	Frederick Henry Taylor.
Herbert C. Oates.	Frank Wallis.

Mr. Richard Aylmer and Mr. H. Koerner were appointed scrutineers of the ballot.

The CHAIRMAN: I have now the pleasure to present the various premiums. The "Institution Premium," value £10, has been awarded to Mr. Nikola Tesla. It consists of 22 volumes of books, which have been selected, at Mr. Tesla's request, by Professor Dewar. I am only too sorry it is not in my power to hand these books to Mr. Tesla himself; they will be forwarded to him by the Secretary.

The "Paris Electrical Exhibition Premium," value £5, has been awarded to Mr. Anthony Reckenzaun; it consists of four volumes, and a framed copy of Messrs. Barraud's photograph of the members of the Institution.

The "Fahie Premium," value £5, has been awarded to Mr. A. W. Heaviside and Mr. R. C. Jackson, who also each take a framed copy of that photograph. These photographs, I may say, have been selected by those gentlemen themselves.

The premiums were then presented by the Chairman to Messrs. Reckenzaun, Heaviside, and Jackson respectively.

The CHAIRMAN: It is now my duty to abdicate; and in committing this rather unhappy process of the "happy despatch," I am cheered by the recollection that my successor, Mr. Preece, is undoubtedly the most popular man in the whole electrical engineering profession. The king is dead: *vive le roi.*

Mr. Preece then took the chair.

The PRESIDENT: Before I commence my arduous task, I will ask Mr. Alexander Siemens to perform an agreeable function.

Mr. ALEXANDER SIEMENS: Gentlemen,—It has always been the custom in this Institution, at the changing of Presidents, that the thanks of the Institution are given to the outgoing President, and to me has fallen the task of proposing a formal vote of thanks in this instance. I think this task is an easy one. You will all agree that we leave a very heavy year behind us: we have had more meetings, and they have been better attended, than ever before; and, as far as I know, our President has never been absent from a single one. In the same way, we have had many important committees appointed by the Council, and of all these the President is, *ex officio*, a member: as far as those committees are concerned in which I was myself a member, and from hearsay about the others, I believe Professor Ayrton has not even missed a single committee meeting. Then, as always, there are three good things: those of you who attended the annual dinner will remember in what a happy way he proposed the toasts at that dinner, and in what a pleasant manner the whole dinner passed off, thanks greatly to the ability with which he presided. For all these things I have great pleasure in proposing—"That "the hearty thanks of the Institution are due to Professor Ayrton "for the indefatigable manner in which he has discharged the "duties of President during the past year."

Professor GEORGE FORBES: I have the greatest pleasure in seconding this motion. I can endorse most thoroughly the sentiment expressed by Mr. Siemens as to the diligence and assiduity with which Professor Ayrton has torn himself away from his numerous engagements and valuable work to devote himself to the interests of the Institution. I think we ought to be proud of having had him as our President, and I have the greatest pleasure in seconding this motion.

The motion having been carried by acclamation,

Professor AYRTON said: I thank you very much. I will not eke out my presidential life by making one additional remark.

The PRESIDENT then delivered his Address.

INAUGURAL ADDRESS.

By WILLIAM HENRY PREECE, F.R.S., President.

I.—TELEGRAPHY.

Having completed my fortieth year of continuous service in developing the practical applications of electricity for the use and convenience of man, it has appeared to me that I could not better repay the high compliment you have conferred on me by electing me, for the second time, to be your President than by surveying and criticising the growth of the various branches of electrical industry with which I have been more or less associated during this long period.

In 1852, when I commenced my engineering career in the office of Mr. Edwin Clark, Member of the Institution of Civil Engineers, the electric telegraph was in its infancy—the needle system of Cooke and Wheatstone in England, the electro-magnetic recorder of Morse in America, the sounder of Steinheil in Germany, and the semaphore of Breguet in France, were struggling into existence against adversity and ignorance. They were crude in their form, slow in their action, and uncertain in their working.

The acquisition of practical experience, the reforming tendency of inventive genius, the development of commercial business, the wants of different countries, gradually eliminated the weak, encouraged the strong, and promoted novelty. The fittest survived, but even 40 years has not seen finality. We are improving and advancing as fast now as we ever were.

In 1852 the principal instrument used in England was the double needle. It required two wires for its operation. Our offices were fitted with massive Greek temples, fashioned in wood, containing in their interiors the galvanometers and commutators by which the elements of language were to be transmitted instantaneously between London and Edinburgh and other towns. This wonderful structure, so much prized in its day,

is now, like some antediluvian monster, discovered only in our museums; but its memory is dear to those who, like myself, struggled with its troubles, and who learnt most of what they know by mastering its whims and conquering its vagaries.

The semaphore has also been relegated to the museum, and the Morse recorder is gradually disappearing to the same refuge, but its struggle for existence is more prolonged and better maintained. The telegraphic tory who sees virtue in a tape, and believes in the greater accuracy of a record, still exists in large numbers in various parts of Europe; but he has disappeared from the United States of America and from England. The ear is more accurate than the eye, and more rapid in deciphering those fleeting signals that convey language.

The record, however, remains in Wheatstone's automatic apparatus and in Professor Hughes's beautiful typewriter, which delivers the message to the receiver in clear, bold, Roman characters—an instrument that has weathered every storm, and is now the great international medium of communication between all European nations.

The peculiarity of the telegraph is its great cosmopolitan character. It unites all nations by one language, and connects them together by the same metallic nerves. It has broken down Chauvinistic doctrines and national prejudices. In England we care not whence invention springs: a new process or an evident improvement is as welcome from Timbuctoo as from St. Martin's-le-Grand.

The instrument that we have principally developed in England is the automatic fast-speed apparatus, based on a principle of preparing messages for transmission by punching, devised by Alexander Bain in 1848, and improved in its mechanical details by Mr. Augustus Stroh in 1866.

This has been my special pet, and with the electrical assistance of Mr. J. B. Chapman, and the mechanical skill of Mr. J. W. Willmot, all the ills that telegraphs are heir to have been routed, and the practical speed of working has been multiplied more than six-fold.

It has been one long continual contest between patient

observation, inventive skill, careful experiment, and technical acquirements on the one hand, and resistance, electrostatic capacity, inertia (electro-magnetic and mechanical), bad insulation, impure materials, imperfect workmanship, &c., on the other. But we have, step by step, won all along the line: 75 words per minute have become 500; a possible 130 has become an actual 600. Duplex automatic working over cable lines is possible, and modes of working have been introduced that were thought at one time chimerical and impossible.

All these things have been done without recourse to the Patent Office. Dozens of improvements have been effected which in ordinary commercial life would have been patented and published, and received recognition. The absence of a patent has led to the belief in the absence of invention. An actual six-fold improved capacity for carrying messages is an answer to this supposition.

The Hughes typewriter has this year been successfully duplexed between London and the Continent—a problem which had hitherto baffled all attempts at a satisfactory solution.

The use of electro-motors instead of weights, on this instrument and on the automatic apparatus, and the extensive use of accumulators instead of primary batteries, may be mentioned, although time will not permit me to refer to a host of minor improvements which have been effected.

The results to which I have referred have not been attained without very special attention to questions of construction and maintenance of the wires, both aerial and submarine, and a very complete system of test is now applied both before and after every line is completed.

In the early days of telegraphic communication very rough and crude tests were in use, and the condition of the lines caused serious difficulties; but at the present day we must ascertain the purity of the metal employed, its mechanical strength, its electrical resistance and capacity, its insulation resistance, and the relationship between the latter and the conductor resistance, as well as its speed value.

The employment of copper as the conductor suspended on

poles in place of iron, which was inaugurated at my instigation in 1884, by a very costly experiment between London and Newcastle, has had a material influence in increasing the speed of working and improving telegraphy in all branches.

This is due not only to its reduced resistance, but to the absence of electro-magnetic inertia in a long, single-suspended copper wire. All our long important telegraphic and telephonic circuits are now built with copper.

One of the arguments used against the proposed transfer of the telegraphs to the State was the notion that invention would not be fostered by a Government Department. This has been entirely falsified. Telegraphy has been advanced in this country more rapidly by the British Post Office than by any private undertaking, and we have certainly shot ahead of our smart cousins on the other side of the Atlantic, from whom, however, I am proud to say, I learnt so much on my visits in 1877 and 1884. Their engineers are looking to us to develop their inventions, and we have done so. They cannot always get them taken up in the States. Diplex, quadruplex, and multiplex telegraphy are importations from them, but they have been improved in our service by our own developments, and have now become the staple and the standard modes of working. No one has done more to effect this object than Mr. M. Cooper.

An accident in the drafting of the Act of Parliament of 1868-69 transferring the telegraphs from the hands of private companies to that of the State, has led to a tremendous development of newspaper reporting in England. Few people are aware of the immense business done for the Press. The growth of Press messages is shown in the fact that 21,701,968 words paid for in 1871 have grown in 1891 to 600,409,000—an average of nearly 2,000,000 words per day.

When Mr. Gladstone spoke at Newcastle, at the National Liberal Federation, in 1891, 390,778 words were signalled to different parts of the country. This kind of business is not, however, confined to the Post Office. The Exchange Telegraph Company, which commenced operations in 1872, working under the license of the Postmaster-General, has in London over 800

instruments at work (120 being in newspaper offices), distributing a daily average of 3,381,134 words to various receiving instruments, adapted to the requirements of the respective services; the financial intelligence, for example, being transmitted over instruments furnished with type-wheels containing the various fractions most in use in Stock Exchange quotations. The latest form of this instrument prints at the rate of 40 words per minute. General and Parliamentary intelligence are distributed to the clubs over column printers, and legal, sporting, and Parliamentary news to newspapers on specially fast tape printers, capable of delivering, in the hands of skilled operators, 45 full words per minute to any number of subscribers simultaneously. The news transmitted is chiefly commercial and financial, amounting to 2,775,000 words per day.

To return to the purely State telegraphy. Some idea of the growth of the general telegraphic business of the country may be gathered from the following statement, which gives the total number of messages paid for in each year:—

1852	211,137
1869	6,830,000
Transfer took place in 1870.	
1882	31,345,861
1892	70,215,439

The monopoly of the Post Office is a mutual co-operative concern, in which every member of the community has an interest, and which everyone helps to maintain in efficiency and good working order by watchful supervision. It is not a mere commercial industry, maintained for the benefit of the few who have risked their capital in speculation. It is maintained for the general good of the community, and for the public service. Everyone can criticise and growl, and everyone does when he gets the chance, and would probably growl if he had not the chance. There is always joy in the soul of the editor when he fancies he discovers a fault in a Government official. We public servants do not object. We rely on public criticism, and our sole object is to serve our masters, with a con

scientious determination to do our duty to the best of our ability.

II.—Submarine Telegraphy.

In 1852 the only working cable was that laid between Dover and Calais by the Submarine Telegraph Company, under the auspices of Sir James Carmichael (one had been laid between Anglesea and Ireland, but failed on the second day). Those between Dover and Ostend, Orfordness and Holland, and between Scotland and the North of Ireland followed in 1853; and several miles of the first-named cable have remained down ever since, and form parts of the present cable between Ramsgate and Ostend.

In those early days I was much engaged in testing, laying, and maintaining submarine cables, and in 1858-62 was engineer to the Channel Islands Telegraph Company, whose cable between Portland and Alderney gave such immense trouble, from the nature of the ground near either end, that it had eventually to be abandoned, because the business did not pay for the cost of maintenance. A new cable was laid in 1870 over a different and better route, and now, 1893, the islands maintain cables of three conductors.

The growth of submarine cables about the world is extraordinary. The total mileage, which in 1852 was 87 nautical miles (nauts)*, now reaches a total of 139,594 nauts, of which 14,479 nauts belong to various Government administrations.

The experience of early days has taught us how essential it is to have accurate surveys of the bottoms to be crossed before trusting cables to their tender mercies; and thus geography and biology have been immensely benefited by the work of the electrical engineer.

By the end of 1855 the North American lines had extended to Newfoundland, while those in Europe reached the West of Ireland, and a scheme was started to connect, by a cable across the Atlantic, the New World and the Old. Cyrus Field, as

* It is a very general custom to spell this abbreviated term for a nautical mile knot, but a knot is a velocity, not a length. A knot is a nautical mile per hour.

Vice-President of the New York and Newfoundland Telegraph Company, obtained the sole right to land cables in Newfoundland, Nova Scotia, Prince Edward's Island, and the State of Maine, and the Atlantic Telegraph Company was formed by him to make and lay the cable.

The first attempt was made in August, 1857, by H.M.S. "Agamemnon" and the U.S. frigate "Niagara;" but after paying out about 380 miles from the Valentia end the cable broke and the expedition was abandoned until 1858, when the same two ships, this time commencing in mid-Atlantic and steaming in opposite directions, succeeded in laying the cable, but after 732 messages had been sent through it again failed, in October, 1858. Several unsuccessful attempts were made to pick it up, and the Atlantic scheme remained in abeyance until 1865, when a heavier cable was successfully laid, being the commencement of the present Anglo-American Company's system, with its four cables now working across the Atlantic. There are now 11 cables bridging the Atlantic Ocean, 10 of which are duplexed.

Between this country and the Continent all the cables (with the exception of those to Norway, Sweden, and Denmark, those to Spain, and those forming the connections with the Eastern Telegraph Company's system) now belong to the respective Government administrations, and are all maintained by them.

The British Post Office owns and maintains those to Holland and one of those to Germany, while those to Belgium and France, containing 37 wires, are joint property, but maintained by the British Post Office.

By far the greatest cable corporation in the world is the Eastern Telegraph Company, whose system of 25,376 miles stretches from Cornwall to Bombay, connects the northern and southern shores of the Mediterranean with Malta, and joins up the various other islands of the Mediterranean and the Levant. This company, in conjunction with the Eastern Extension and the Eastern and South African Companies, also gains access to Australia and New Zealand on the one hand, and to the Cape of Good Hope on the other, the combined mileage reaching a total of no less than 47,151. This enormous system has all grown up within, practically, the last 23 years.

The form of cable has practically remained unaltered since the original Calais cable was laid in 1851. Various sizes of core and armour, and various modes of protection from decay, have been used to suit different routes, but the cable of to-day may be said to be typically the same as that used in the English Channel in 1851, and in the Atlantic in 1865.

The first cable had gutta-percha as a dielectric, and it is still almost exclusively used for submarine cable core; but the manufacture has so improved in the last 20 years that a core having an insulator weighing 150 lbs. per naut, which then had a dielectric resistance of some 250 megohms a naut at 75° F., can now be obtained giving 2,000 megohms at the same temperature. India-rubber is creeping in, owing to the high price and scarcity of gutta-percha.

Next to strong tides, rocky bottoms, anchors, and shallow water, the greatest enemy to submarine cables, more especially in the tropics, has proved to be the teredo of various species; but this depredatory worm has been utterly routed by covering the gutta-percha core with a lapping of thin brass tape laid on spirally. A remarkable thing about this little insect is that, whereas 20 years ago it was practically unknown in our English waters, it has now gradually spread all round our coasts, with the exception, perhaps, of the North Sea. A new cable about to connect Scotland and Ireland is being served with brass tape.

With the cables has grown up a fleet of telegraph ships to lay and maintain them. In 1853 the "Monarch," belonging to the Electric Telegraph Company, was the only ship permanently employed as a repairing telegraph ship; now, in 1893, the cable fleet of the world numbers no less than 37, of which 7 belong to Government administrations and the rest to private companies, the Eastern Telegraph Company heading the list with 5 vessels.

Perhaps the most remarkable history of a cable is the following:—In 1859 the light cables laid in 1853 from Orfordness to Holland were picked up and replaced by a heavier one. A few nauts were sold to the Isle of Man Telegraph Company, and had an extra sheath laid on. This cable was submerged between that island and St. Bees, where it remained until 1885, when it was

replaced by a three-core cable. It was again put under water in 1886 as part of the cable between Uist and Harris, in the Hebrides, where it still lies, as good as ever. The durability of submarine cables is remarkable. That laid between Beachy Head and Dieppe in 1861 is still working; and that laid between Beachy Head and Havre in 1870 has broken within the last month for the *first* time.

Despite the enormous growth of submarine cables during these 42 years, there would appear to be plenty of scope for still further extension. The Pacific still remains untouched, and a project is at the present time under consideration to connect our possessions in North America with those in Australia.

III.—Lightning Protection.

In 1752 Franklin established the identity of electricity and lightning, and showed how buildings could be protected from the ravages of thunderstorms. This was one of the most beneficent conceptions of scientific practice. The discovery of a cause led to the application of a remedy. Experience has scarcely improved on Franklin's methods. His simple secret was to construct an easy metallic path for the electric charge to flow silently, safely, and imperceptibly to earth, without mechanically rending and tearing the air in one of those long, violent, disruptive, and destructive sparks where energy was suddenly expended in producing heat, explosion, light, and sound. It was prevention, not cure. There is no more perfect apparatus in existence than the lightning protector; and if it ever does fail to do its duty, it fails from man's neglect of some simple rule, or his apathy in maintaining in proper order his uncomplaining slave. Immunity from accident leads to neglect, and neglect breeds danger. Periodic and systematic inspection is the only possible means of preventing decay of effectiveness. In 1892 not one single accident was recorded to any high-class telegraph instrument in the whole of the United Kingdom. Professor Oliver Lodge has, with characteristic energy, endeavoured to modify our views as to the behaviour of lightning discharges and as to the form of protectors, but without much

success. His views have not received general acceptance, for they are contrary to fact and to experience.

IV.—Railway Signalling.

The application of electricity to block signalling is marked by three distinct epochs—its first inception, its popularisation, and its combination with the mechanical signals.

In 1852 the traffic on our railways was regulated by time and sight, and the safety of the railway traveller mainly depended on the watchfulness of the driver. The difficulties which attended the working of the enormous traffic with which the London and North Western Railway had to cope, during the great Exhibition of 1851, induced that company to seek the aid of electricity. It was then that Mr. Edwin Clark, employing for the purpose the double-needle form of telegraph in use, devised what may be regarded as the parent block system; and it became one of my earliest duties to work it out and to apply it. In 1854 the Great Northern adopted it, and it is interesting to note that, although its application was for a considerable period exceedingly limited, in character and principle it remains to-day very much what it was when first established.

In 1861–62 it was my privilege to become still more intimately connected with block signalling, for at that period I developed on the London and South Western Railway the system which is known by my name. The characteristic of this system is the assimilation of the outdoor to the indoor signals, thus placing in the hands of the signalman a counterpart of the signals worked by him for the guidance of the engine-driver—the same semaphore signals, the same kind of interlocking switches, and the same mode of working. My object was the provision, not only of a reliable system—one which should record its own action —but also one which should popularise block working, and which should in a great measure dispense with that course of instruction which, at that time, formed one of the impediments to its more rapid adoption. Walker and Tyer worked in the same field; while Mr. Spagnoletti sought a similar result by employing, in

the place of the indications of the needle, a movable flag inscribed "Line clear," "Line blocked."

Great as are the advantages of the block system, as it is popularly known, there is yet room for improvement. Over thousands of miles of railway the electrical signals are worked independent of the mechanical signals. It is possible to operate the one without the other. The desirability of welding the two together, so as to render them perfectly homogeneous in their action, was recognised by me as early as 1870, when what I believe to be the first interlocking of the electrical with the mechanical signals was effected at Southampton, and subsequently at Wimbledon and Kew, on the London and South Western Railway. At this time the transference of the telegraphs to the State called for my services in another direction, and the subject was dropped. In 1875–77 the combination was, however, most effectually completed by Mr. Sykes, who has now brought his system to great perfection. He was followed, in 1877–78, by Saxby & Farmer, under Hodgson's patent; and later still by Mr. Langdon, on the Midland Railway, and by others. It now only remains for the railway companies to apply such of these inventions as may be most suitable to their respective systems, in order to place block working upon a perfectly sound basis.

The latest application in this now extensive industry is Tyer's Train Tablet System, produced in 1878; together with that of the Electrical Train Staff, devised and elaborated by Messrs. Webb and Thompson, of the London and North Western Railway, in 1889. Both systems tend towards the modification of the difficulties, as well as the cost, attending the working of single lines of railway.

To railways, electricity has proved an invaluable adjunct. In the repetition of signals obscured from the view of the signalman, introduced by me in 1862; of that of the light which forms the night signal, introduced by Warwick about the same date; of telephony, now becoming daily more and more indispensable—electricity plays a quiet, unobtrusive part, the value of which is only fully felt when, from some cause, its use has to be temporarily suspended.

A possible further field for the employment of electricity in railway working may yet be found in automatic signalling. In the United States it has been so employed to a great extent. In England it has, so far, failed to find a footing. Here, railway managers appear to be averse to the employment of any system which fails to fix the responsibility of failure which may lead to disaster.

The block system is now, by Act of Parliament, compulsory on all British railways; and that mode of working, which at its inception worked its way so slowly into use, now controls the traffic which daily passes over 28,000 miles of line, and plays not only an important part in the conduct of railway traffic generally, but proves to be the chief factor in the prevention of accident and the protection of life. Without its aid it would be utterly impossible to deal with the railway traffic of the present day.

The enormous development of electrical engineering upon our railways is shown by the following return from one system only:—

Midland Railway Telegraph Plant, December 31*st*, 1892.

Approximate.

S.N. Block Instruments	4,350
Block Bells	2,645
Signal Repeaters	2,500
Light Repeaters	640
Message Instruments	1,736
Instruments of other kinds	3,108
	14,979
Mileage of Wire for Block	5,712
,, ,, other purposes* ...	5,582
	10,294

* Exclusive of P.O. wires.

V.—TELEPHONY.

I had the good fortune in 1877 to bring to England the first

pair of practical telephones. They had been given to me in New York by Graham Bell himself. After a series of experiments, I brought them before the British Association meeting, which was held that year at Plymouth. Who at that time could have imagined that the instruments, which were then but toys, would, within 16 years, have become a necessity of commercial, and almost of domestic, life? Yet to-day the number of telephones in actual use may pretty safely be put down at a million!

During 1878 Edison devised his carbon transmitter, and Professor Hughes presented his "microphone" to the world. These inventions made the telephone a practical instrument of vast commercial importance. It may be said to have sprung into existence well-nigh perfect; and the fewness of the actual improvements on the Bell receiver and the Hughes microphone is scarcely more astonishing than the immense number of fruitless attempts at improvement that have been made. Even now the original instruments are not easily beaten.

The institution of telephone Exchanges has led to a development of systems of switching that might fairly be considered a special study in themselves, and the demand for communication between distant places has necessitated the application of much special attention to the method of constructing lines and of arranging circuits.

It is in this latter field that I have been a diligent worker, and the application of the so-called "K R" law has proved of material benefit in connection with the problems of long-distance telephony. It is a law which implies that the number of signals that can be transmitted per second through any circuit depends solely on the capacity (K) and the resistance (R) of the circuit. It is very much the fashion to deny the accuracy of the K R law. This is probably the result of ignorance of its meaning or of its interpretation. Some speak of it as empirical, others scoff at it as imaginary, and some sneer at it as an impossible law; but it is a law that has determined the dimensions and speed of working of all our long submarine cables; it determines the number of arms a circuit can carry on the multiplex system, the speed attainable with the Wheatstone

system, and the distance to which it is possible to work quadruplex; it is a law that has enabled us to bring London and Paris within clear telephone speech of each other, and which will probably before the year is out, enable Dublin and Belfast to speak to London—a message of peace to Ireland as solid and substantial as any promised political proposal.

The New York and Chicago trunk line is 950 miles long, and it is built with 435 lbs. (or No. 8 S.W.G.) copper wire. This wire gives a resistance of 2·06 ohms per mile, which is easily verified; but it is said by Mr. Wetzler to have a capacity of 0·0158 microfarad per mile, which cannot be verified, and which is absurdly high. 0·0158 microfarad was a measurement made by me in England on an old line, but I have frequently pointed out that owing to the use of earth wires the capacity of our English lines is very much greater than that of American lines. Mr. Edison discovered this in 1872 when he came to England to introduce his automatic system. Moreover, I have also pointed out that induction still further diminishes this capacity. The capacity of the Paris circuit does not exceed 0·005 microfarad per mile. I should estimate the Chicago circuit at 0·004 microfarad per mile, and the K R at 7,500, which gives a result that quite accords with the opinions that I have heard expressed by those who have tried the two circuits as to the relative efficiency of the Paris and Chicago lines. My American friends would have done better if they had used thicker wire. I should have specified 600 lbs. per mile; but if it had been in England, I should have used 1,000 lbs., for we cannot dispense entirely with cables and underground work as they have done in the States, and the increased capacity introduced must be compensated for by reduced resistance. As a matter of fact, I once proposed 1,200 lbs. wire for a circuit between London and Berlin—a distance of 760 miles, including a cable 55 miles long.

The beneficial effect of induction as a negative capacity is also observed when working a circuit telegraphically with automatic high-speed apparatus. Thus, on two copper wires 450 miles long, making 900 miles altogether, the speed on each single wire was 120 words per minute, and on metallic circuit—

Loop *via* different routes	...	120	words per minute.
" on same poles... ...	...	150	" "

So that the improvement effected by induction was 25 per cent.

There is no difficulty in measuring R of a metallic loop. The Wheatstone bridge determines it at once. There is more difficulty in obtaining K. It cannot be measured directly. But with a metallic loop of copper, partly overhead and partly underground, there are several modifications required, due to electrostatic and electro-magnetic induction, which are at present beyond the reach of formulæ, and render it difficult to determine the capacity except approximately from the telephonic effects themselves. Thus the capacity on the London-Paris circuit proved to be only one-half of that obtained by calculation, and every long circuit will require its own K to be determined by comparison with an empirical K R scale. Such a scale I have determined by careful experiment on artificial cables.

I have recently devised a new form of cable which will probably quadruple the rate of telegraph working to America; and I may say with all confidence that there is no theoretical reason whatever why we should not converse between London and every capital in Europe, while it is not impossible to speak even across the Atlantic.

VI.—Domestic Applications.

The era of the application of electricity to domestic purposes may practically be said to date from about the year 1850, when Hipp, of Neufchatel, introduced into the Berne Chamber of Deputies a system of electric bells. The system was taken up by Breguet, of Paris, and largely introduced by him in France. The system, as arranged by that inventor, was brought to England by me in 1863, and taken up by Messrs. Adams, the well-known firm of ironmongers of the Haymarket. Although some time elapsed before the system took a firm hold, its subsequent progress was very rapid, and now forms a very flourishing branch of electrical industry.

There can be but little doubt but that the success which electric bells for domestic purposes has attained has been mainly

due to the introduction, in 1866, of the Leclanché battery. The perpetual attention which Marié Davy and Daniell cells (the form of batteries at first employed for the purpose) required must have very seriously, if not fatally, hampered the march of the most successful of the domestic applications of electricity, had the invention of the Leclanché battery not come to the rescue.

Fire and burglar alarms, as applied to houses and worked by electric action, to some extent preceded the introduction of the electric bell system, and many exhibits of these appliances were made from time to time; but their success has been very partial, and chiefly, no doubt, because these appliances, being very seldom required for use, get out of repair, and have an unfortunate trick of failing exactly at the time when they are most required.

Water-level apparatus, though perhaps not strictly classifiable with domestic appliances, in so far as they are closely connected with water supply, may be alluded to. In the International Exhibition of 1862 an apparatus of this kind, devised by MM. Mouillon and Vinay, was shown in action; and within the last 15 years the Postal Telegraph Department has fitted and maintained with great success special forms of instruments, designed and worked out by Mr. H. R. Kempe, for various water companies.

Electric clocks have been a very favourite subject for invention, and were introduced in the very early days of the electric telegraph; an enumeration of the patents taken out for these appliances would run to a very long list, but the record, with a few marked exceptions, is practically one of failure, representing much ingenuity but little real utility. We use in several post offices systems of electrically controlled clocks that work very well.

Electric hoists, devised by Siemens, Hopkinson, Crompton, and others, have been used to some small extent; but their introduction has not been so extensive as might have been anticipated. One cause of this is, no doubt, the want of a good self-starting alternate-current motor; but it is more especially due to the question whether at present high prices electric energy for the purpose can financially compete with the generally adopted means of working the hoists.

For ventilation purposes on a small scale, the adoption of fans for the purpose should prove of considerable value, as such fans can be placed in any convenient place out of sight and be contained in a very small compass.

Heating and cooking apparatus worked by electricity have not at present a very favourable outlook, though many appliances of the kind have been shown in operation, especially at the recent Crystal Palace Exhibition. The question is far more a financial one than anything else, and the public have not yet been educated up to the point of paying heavily for "*biftek à la Française*" because it is cooked on highly scientific principles. I have in use in my house a flat-iron, a tea kettle, and a curling-tongs heater, each heated electrically.

I must not omit to mention the large use of "electric gas-"lighters," an invention which has hardly received the full credit which it deserves.

VII.—Electro-chemical Industry.

The electro-chemical industry is, financially, one of the most important in this country. Electro-plating was started by Elkington at Birmingham in 1840. There are now about 170 electro-platers in Sheffield, and over 100 in Birmingham. Christophle, of Paris, deposits annually six tons of silver.

Nearly the whole of the copper used for conducting purposes in the various applications of electricity is extracted from its impure ores by electro-deposition. 0·3 H.P. continuously applied deposits 1 lb. of copper per hour, and this costs about 0·1d., so that the process is very cheap while the effect is wonderful, for copper is now frequently supplied having a resistance of 102 per cent. of pure copper. This is due to the fact that the specific gravity of the material has been raised from 8·90, at which Matthiessen took it in 1862, to 8·95, which is now obtained with hard-drawn electro-deposited material.

Aluminium is being extracted in large quantities from cryolite and bauxite in England and in the United States (Pittsburg). In Switzerland water power at Neuhausen—the falls of the Rhine

—is being very successfully employed for the same purpose. The price of the metal has, in consequence, fallen very much, but the demand is greater than the supply, and the price is rising.

Many attempts have been made to produce caustic soda and chlorine from common salt, especially for bleaching purposes in the paper industry; but all the difficulties in the case have not yet been surmounted, the principal one being the destruction of the anodes—at any rate, the industry is not a staple one at present. An interesting process, by which phosphorus was separated from corundrum, a mineral phosphate, by the dissociating effect of the terrific heat of the electric arc, was developed by Readman, Parker, and Robinson, at Wednesfield, near Wolverhampton; but though the right to use this mode of producing phosphorus was purchased at a very high figure by those who have the monopoly of that business, it does not appear that it has been put to practical use at present. Great advances and developments are being made in other directions, notably in the production of ozone, in electrical bleaching, for tanning, and in reduction by the electrical furnace.

There are few oxidising and reducing processes in chemical industry which cannot be effected economically and with advantage by the electric current, and the extreme cheapness with which electrical energy can be manufactured when used continuously, and when produced by falling water, as well as the extreme purity of the resulting deposits, are calling earnest attention to this lucrative branch of the business. It is not impossible to produce a kilowatt-hour of electrical energy by means of water power for 0·1d., and when this is stated to be equivalent to gas used for illuminating purposes at 0·6d. per 1,000 cubic feet, it will be seen what a potentiality of wealth there is in this (at present) much-neglected waste energy of Nature.

VIII.—Electric Lighting.

Although the arc lamp was invented very early in the century, and the glow lamp made a fitful appearance in 1845, it was not until Jablochkoff startled the world with his candle in 1877, and Edison and Swan perfected the carbon filament of

high resistance in 1880-81, that the electric light began to attain its practical stage. Its rapid development has been seriously retarded in England by the operations of a monster called into existence by the limited liability legislation of recent years—the rapacious financial promoter—whose plunder in one year of our period far exceeds in amount the sum of all the thefts of all the highwaymen and burglars that were ever hanged. He has ruined the prospects of private enterprise, and has rendered absolutely necessary the Acts of 1882-88, which have thrown the industry into the hands of the local authorities of our cities and boroughs.

The glow lamp has been greatly improved, and as this year will witness the expiry of the controlling master patent, we shall see the price reduced to something not far from 1s. Some of us can remember the time when the price was 25s. It has now for some time been 3s. 9d. As a glow lamp costs only 9½d. to make, we may reasonably hope that my anticipation will be realised.

The cost of wiring is a great deterrent to the introduction of the electric light into small tenements and into houses held on short tenures. The materials used must be of the most perfect kind, otherwise insecurity is incurred; but there are expensive practices of wood casing and of ornamentation which want reform. The Cheap Jack is the curse of the industry, and the absence of specification and of inspection, the danger of the user. The fire insurance offices have maintained our practice in England pretty secure, but in other countries the character of the work has been very indifferent.

The problem of economic distribution has been much simplified by the introduction of the three-wire system by Dr. John Hopkinson, and by the improvements made in high-pressure apparatus. High pressure not only economises the heavy cost of mains, but simplifies immensely the mode of regulation. The early pioneer alternate-current systems, which have done so much to further the development of electric lighting, were, however, costly in their coal bills, owing to great losses of energy in their iron and copper, but the transformer of the present day is as perfect in efficiency as the dynamo. These losses have practically

disappeared, and modes of switching transformers in and out at will, have removed every difference of cost between high-pressure and low-pressure systems. The prejudice against high pressure is still strong; it is thought to be unsafe, but time and experience will eradicate this impression as they ultimately eradicate every fallacy. The prejudices against high-pressure steam and against high speed on railways were once equally strong, but those prejudices survive only as "bees in the bonnets" of a few.

The progress in electric lighting is now principally economical. The cost of production of electrical energy is rapidly coming down. Elements of waste are being eliminated, and continuous working throughout the day and night is being encouraged. If a full load could be maintained during the whole 24 hours, electrical energy could be manufactured for one-third of a penny per supply unit, and this is equivalent to gas at 2d. per 1,000 cubic feet. The potentiality of economy in electric lighting is thus beyond the dreams of the gasman. The most marked features in the development of this great industry are the ostrich-like blindness of the gas manager, who buries his foresight in his fat dividend, and the childish wailings of gas technical journalists. The gas industry should itself have nursed this herculean infant who is rapidly strangling one of its main sources of profit. Fortunately for the gas shareholder, there are other sources of income—warming, cooking, motive power—to be developed. Our corporations and local authorities are showing more astuteness. Manchester, Nottingham, Dewsbury, though successful gas producers, have assumed also the position of undertakers of electrical energy.

Monopolies are held in pious horror by the critical public, but it fails to draw a distinction between a monopoly held by the few for their own personal benefit at the expense of the many, and that held by the many for their own purposes, comforts, and economies. Electric lighting, carried out under the provisions of the Acts of 1882 and 1888, is a self-supporting, self-managed, and profitable industry. It is a business of a mutually co-operative character, carried on by the ratepayers themselves for their own sole benefit.

The bright illumination of our streets increases their security, not only from crime, but from accident. Accidents in large towns are relatively greater during the hours of darkness than during daylight. The amount of light distributed over the roadway by an ordinary street gas-burner is very small, but arc lamps can be so distributed as to rival daylight in their illumination. If any-one wants to know what can be done in this direction, let him visit Spiers & Pond's establishments in Water Lane, Blackfriars.

The rate of growth of the industry is shown by the following returns:—

	London.
Dec. 31, 1890	145,000 electric lamps fixed.
„ 1891	330,000 „ „
„ 1892	500,000 „ „

	Bradford. (Lamps.)	St. Pancras. (Lamps.)	Brighton. (Lamps.)
1889	1,000	—	—
1890	8,467	—	—
1891	15,000	13,000	2,572
1892	20,000	21,000	10,248

Nothing can stop the growth, or prevent the rapid spread of, this beneficent and sanitary mode of illumination into the confined, ill-ventilated, overcrowded homes of the working population. The electric light is essentially the poor man's lamp.

Many efforts are being made to utilise the waste forces of Nature in producing electric currents for the economical supply of the light. In America, Scotland, Switzerland, Italy, and, indeed, wherever waterfalls are available, electric plant is being installed to convert the energy of the fall into the useful form of electricity. At Tivoli, near Rome, a fall of 165 feet is used to work six turbines of 350 H.P. each, giving 2,100 H.P. in all. Six high-pressure alternators working in parallel send electrical energy at over 5,000 volts pressure to Porta Pia, near Rome, 14·8 miles from Tivoli, through four stranded copper conductors, each having a diameter of 13 mm., and bunched into one metallic loop, giving a total resistance of 4 ohms. At Porta Pia the 5,000 volts are reduced to 2,000, and the currents are distributed to several sub-

stations spread over the city, where they are again lowered to the safe pressure of 102 volts, at which voltage the current is supplied to the consumer on the three-wire system. There are 600 arcs and 30,000 glow lamps in use in Rome, but they are not all supplied from Tivoli. I inspected this installation only a few days ago, and found everything working smoothly and efficiently under the able guidance of Professor Mengarini.

Water power abolishes the coal bill, but it must be remembered that the cost of maintenance of machinery and of the erection and upkeep of conductors, limits the distance to which the energy of falling water can be economically transmitted. The proposal to light New York by currents generated at Niagara is at present financially absurd. It is doubtful whether it will be commercially advantageous at Buffalo, 30 miles away, but it is certain that at Tivoli it can be so applied with advantage and profit.

There is much water power in this country that might be usefully employed. At Worcester it is proposed to use the water of the Teme, a tributary of the Severn, to supply electrical energy to the city—an experiment that will be watched with considerable interest, for the use of water power will solve the difficulty occasioned by light loads during the small hours and daylight. Keswick and Lynton have already been so served, but on a small scale only.

There are many towns whose public streets could be brilliantly illuminated by the streams running past them, but there is much fear and distrust to be removed from the minds of our local magnates, and a considerable amount of education necessary before the public will receive the full value of the gifts that Nature so freely places at its disposal, and which the engineer so thoroughly converts into a utilitarian form.

The rippling brook, the rhythm of falling water, the flowing tide, and the stream that rushes wildly over well-worn boulders, have furnished the poets of all ages with imagery and with illustration.

> "All Nature rejoices
> And moves with a musical flow."

But no poetry of the past surpasses the poetry of the modern

engineer, who transforms this musical flow into a gentle form of silent energy that answers the first fiat, "Let there be light," and that gives comfort to the home, health to the being, and security to the worker.

IX.—Power Transmission.

The oldest proposal for transmitting power electrically over a distance is probably that contained in the patent specifications of H. Pinkus, who proposed in 1840 to work an electric railway by power obtained from large primary batteries established on the line, but not carried on the trains. Pinkus must have contemplated transmission of current by wire. The first time power transmission was publicly shown was by Fontaine in the summer of 1873 at the Vienna Exhibition; but whether the discovery of the reversibility of the dynamo and of the possibility of transmitting energy from a generator to a motor through a long length of cable was purely an accident or the outcome of conscious investigations is not known. The story is told of an attendant who, when tidying up the stall of the Gramme Company, saw two cable ends lying loose, and, thinking they belonged to a Gramme dynamo near by, inserted them into its terminals. The machine immediately began to revolve, and it was then found that the other ends of the pair of cables were attached to another machine which was working. The transmission of power by means of two Gramme dynamos was also shown at the exhibition of scientific apparatus in South Kensington in 1876. Although experiments of this kind were repeated by various scientists, the discovery seems to have been left undeveloped until Messrs. Chretien and Felix applied it, in 1878, on a practical scale for working an elevator, and in 1879 to agricultural work. The generator was a Gramme dynamo at the Lermaise sugar factory, and the energy was used for ploughing a field half a mile away at the rate of 200 square feet per minute, which corresponds to about 8 H.P. in the plough rope. The first electric tramway was that shown by Siemens in 1881 at the Electrical Exhibition in Paris.

Since then the industry has begun to develop, and many electrical engineering firms took up power transmission. In 1882

Mr. Gisbert Kapp discovered that it was possible to get constant speed at the motor under extreme variations of load if the generator and motor were series machines and had characteristic curves which corresponded to each other. He applied the discovery for working the pattern-makers' shop at Messrs. Crompton's works at Chelmsford, where the load was, of course, liable to great variations. This principle has since then been used by Brown and others in a number of power plants on the Continent. It is also used for coal-cutting machinery, working machine tools, travellers' and other shop appliances, and in Messrs. Denny's shipyards at Dumbarton there is an extensive system of circuits with terminals so placed that work may be done by electric machine tools on any vessel in the yard. In 1886 Mr. Mordey pointed out that a shunt motor regulates for constant speed under varying load if the supply voltage is kept constant. France has very much developed this industry; in Italy we have magnificent examples in Genoa and Tivoli; but it is in Switzerland, where water power is so abundant, that it has flourished so well.

Ferraris and Tesla in 1888 discovered, independently, the rotary field motor worked by multiphase alternating currents. With this discovery the range of transmission has been enormously extended, since much higher voltages than are possible with continuous currents can be employed. Meanwhile, power transmission by single-phase alternating current has also been developed, the earliest example of magnitude being the electric lighting station at Cassel, designed by Oscar von Miller, and started in the beginning of 1891. The Heilbronn three-phase transmission, started a year ago, is working most satisfactorily for distribution of light and power. Inventors are, however, busy on single-phase alternating-current motors, so that existing alternating-current stations may distribute power as well as light, and Messrs. Ganz & Co., Messrs. Brown & Boveri, and the Œrlikon Works have already produced serviceable motors working by single-phase current. Only a few weeks ago a single-phase current motor was installed at the printing works of a newspaper in Chur and connected with the town mains. The paper is now

printed by electric energy brought from a waterfall several miles away. The use of electrically transmitted power in mines has greatly increased during the last few years, especially in America. In Ireland, the falls of Bushmills work the electric tramway from Portrush to the Giant's Causeway, and those of Bessbrook the line from Newry to Bessbrook.

We are anticipating considerable developments in this direction in Niagara, but nothing electrical has yet been done there. It is a subject of congratulation, however, to this country, and especially to this Institution, that one of its Members of Council —Professor George Forbes—has been appointed the engineer to carry out the electrical work. We may probably see power transmission developed by alternating currents in this district on a scale beyond the dreams of science, and voltages of from 5,000 to 10,000 volts handled with ease and safety.

X.—Electric Traction.

The use of electrical energy for working railways is making gigantic progress in the United States, and has commenced to make a serious move in the United Kingdom. The City and South London Electric Railway has proved a decided success, and next week will record the opening of a still larger enterprise on the elevated railway of the Mersey Docks in Liverpool. When last I visited America, in 1884, there was only one electric railway in work, and that was in Cleveland. One company alone, the General Electric Company of the United States, working the Thomson-Houston and Edison systems, has now—

Roads in operation and under contract	440
Cars	8,856
Miles of line in operation	4,628

Thus the average length of electric railway is 10·5 miles, and the average number of motor cars in use about two per mile. The power used is over 100,000 H.P. Speeds of 40 miles an hour are sometimes obtained in the open country, but in the crowded streets this rate is much reduced.

In 1892 there were 250,000,000 passengers carried, 50,000,000 car miles run. The capital invested in such railways is £12,000,000,

and the cost of working comes out at 6d. per car mile. All these railways are worked with overhead conductors, by what are known as trolley lines. Such a line is at work in Leeds, and the South Staffordshire Tramways have just started the same practice.

Considerable experiments have been made to work tram-cars by accumulators carried on the car. They have not met with much success, owing to the cost of renewals. The Birmingham Central Tramways are, however, persevering in this direction, and they are now giving an extensive trial to a new form of accumulator—the Epstein—the proprietors of which have adopted the novel plan of undertaking to renew the accumulators at the rate of 1½d. per car mile.

XI.—Theory.

In the Presidential Address which I delivered to the Society of Telegraph-Engineers and Electricians in 1880, I took the opportunity to formulate the theoretical views of electricity that I had acquired at the feet of Faraday. It is not given to every boy to have his great ambitions realised. One of my ambitions as an earnest listener to Faraday's simple and delightful lectures was to be his assistant, and in almost the last investigation he undertook on electric induction in underground wires, it was my privilege to see much of him, to prepare many experiments for him, and to realise my ambition. Early in 1854, at his wish, I carried out for Mr. Latimer Clark certain experiments on the comparative effect of increments of voltage in increasing the rate of transmission of signals through long telegraph circuits. It was found that variation of voltage had no effect. Currents from 31 and from 500 Daniell cells sent through 768 miles of gutta-percha-covered underground wire showed precisely the same velocity. These experiments were sent by Faraday to Melloni, who had prompted the wish, and Melloni ("Faraday's Researches," vol. iii., page 577) remarked: "The equal velocity of currents of various tensions "offers a fine argument in favour of the opinion of those who "suppose the electric current to be analogous to the vibrations "of air under the action of sonorous bodies." This is to be found in the very last contribution inserted in the greatest

work ever published on our science, "Faraday's Experimental "Researches in Electricity."

Faraday's views were subsequently expounded and extended by Maxwell, who said: "Faraday, in his mind's eye, saw lines "of force traversing all space, where the mathematician saw "centres of force attracting at a distance; Faraday saw a "medium where they saw nothing but distance; Faraday "sought the seat of the phenomena in real actions going on "in the medium, they were satisfied that they had found it "in a power of action at a distance impressed on the electric "fluids" (Maxwell, "Electricity," vol. i., page 10).

Since that period, I have never regarded electricity as anything else but as a form of energy, and its effects as modes of motion of the molecules of matter and of the ether that fills all space; and during my long apprenticeship of 40 years I have never examined one experiment or considered one fact that was not explicable on this mechanical theory.

It must be distinctly understood that I apply the term *electricity* to that form of energy which we as engineers utilise in the service of man. I do not apply it, as some physicists do, to a mere imaginary factor of this energy, sometimes called "quantity," and even honoured with a unit—the *coulomb*. I use it in the same sense that we use the terms *light*, *heat*, and *sound*, which are universally acknowledged to be forms of energy. In my address as President of the Mechanical Section of the British Association at Bath in 1888, I pointed out the great confusion that arose from this misapplication of terms, and I suggested that the disturbing factor—the vague subjective unreality of the physicist—might be called by some other term. I now go further, and suggest that the term be abolished altogether. We can do without it. It is not wanted. It is a myth. We have excluded the coulomb from our new legal system of C.G.S. units.

The electricity of the engineer is a definite form of energy. It has one objective reality that we can generate, utilise, measure, and sell. It is even regulated by Act of Parliament. It is, moreover, "understanded of the people."

The tendency of modern practice is to import simplicity into

theoretical inquiries. True theory does not require the abstruse language of mathematics to make it clear and to render it acceptable. The doctrine of evolution, the principle of the conservation of energy, the undulatory theory of light, the thermal equivalence of work, the development of electro-chemical transformation, and all that is solid and substantial in science and usefully applied in practice, have been made clear by relegating mathematical symbols to their proper store place—the study.

The doctrine of the conservation of energy implies that energy, like matter, cannot be created or destroyed. Its form only can be changed. Its total value always remains the same, and we can always measure it when all the different forms in which it exists in the particular system are taken into consideration. There has been no more fruitful principle introduced into science during this century than the equation of energy, which implies that the total energy of every physical system is the sum of all the energies of its different parts, whether usefully or uselessly expended.

Every molecule at any instant of time is in one place only, and any change of its position during any interval of time is the result of force applied, and is effected along a continuous path and in an orderly manner. The laws governing the mode of motion, and determining the direction, magnitude, and nature of the work done, are the objects of physical research.

Maxwell said: "The special work which lies before the "physical inquirer is the determination of the quantity of energy "which enters or leaves a material system during the passage of "the system from its standard state to any other definite state."

The molecular theory of electricity, which regards it as a form of energy, supplies the physicist with a powerful weapon to combine in one great science of "energetics" all physical phenomena. Maxwell, had he lived, would probably have accomplished this. He raised a superstructure upon the foundation stones laid by Faraday. He annihilated action at a distance, he established the existence of stresses and strains in the ether, and he showed the identity of luminous and electro-magnetic waves.

Subsequent experiments have confirmed his views.

Electricity is therefore energy which is transmitted by matter and through space by certain disturbances the result and the equivalent of work done, and in certain orderly and law-regulated forms, called "electro-magnetic waves." It is not difficult to conceive the ether carved or the molecules of matter swayed or excited in definite periodic waves. A molecule is subject to all kinds of motion—translation, oscillation, rotation upon its own axis, and revolution about some external axis. Clausius (*Pogg. Ann.*, clvi., p. 618) suggested that the atoms or groups of atoms constituting a molecule revolve around one another similarly to planets, and are sometimes nearer to and sometimes further from each other. The difference between the infinitely great and the infinitely little is only one of degree. The motions of the solar system and that of a molecule of water are similar. These motions are imparted to and transmitted by the ether, and they are taken up again by matter. One kind of wave gives us light, another radiant heat, another magnetism, and another electrification. The rate at which these waves move is the same, viz., 30,000,000,000 centimetres, or 192,000 miles, per second. It is only their form and their frequency that differ. Matter and ether are subject to strains, currents, vortices, and undulations, and every single electro-magnetic phenomenon can be compounded of or reduced to one or other of these mechanical disturbances. Rotation in one direction gives positive electrification; rotation in the opposite direction gives negative electrification. A whirl in one direction gives us North magnetism; in another direction, South magnetism. Hertz, the experimental exponent of Maxwell's views, has shown the existence of electro-magnetic waves, and has proved their reflection, refraction, and interference. The rate of their propagation is the same in ether, air, and conducting wires.

The most recent discoveries and deductions are all in accordance with this mechanical theory. J. J. Thomson's views that at high temperatures, in the act of dissociation, all gases, and Dewar and Fleming's conclusion that at low temperatures—in fact, at the absolute zero of temperature—all metals become

perfect conductors, might almost have been predicted. Hysteresis and Foucault losses are mere wastes of energy, due to molecular friction or to internal work done on the molecules, assisted by bad design and impure material; but, being measurable and comprehensible, their reduction to a minimum has become possible and actual.

It is a misfortune that a beautiful hypothesis like Maxwell's electro-magnetic theory of light has been discussed almost solely by mathematicians. Its consideration has been confined to a small and exclusive class. It has not reached the public; and this is to be regretted, for, after all, it is the many, and not the few, that determine the acceptance or refusal of a theory. The existence of the ether is now thoroughly comprehensible. Light is now regarded as an electro-magnetic disturbance. The eye is an extremely sensitive and delicate electro-magnetic instrument. The difference between luminous, thermic, and electro-magnetic waves is one of frequency and form. We thus have to consider the propagation of these waves not only in the conductor and in the dielectric in the direction of the circuit itself, but in the ether at right angles to this direction. The former produces currents in the conductor, and the latter induction and secondary effects in contiguous conductors. Thus it is easy to see why electric and magnetic lines of force are at right angles to each other, and each of them perpendicular to the line of propagation of the primary electro-magnetic wave, and why the transversal disturbances are secondary waves of electro-magnetic energy which can be transformed into electric currents of opposite direction whenever contiguous conductors lie in their path so as to be cut by these lines of force in the proper direction. Induction is thus mere transformation of energy whose direction and magnitude are easily calculated.

It is by following out this line of thought that I have recently succeeded in sending messages by Morse signals across the Bristol Channel between Lavernock and Flat Holm, a distance of 3·1 miles. The electro-magnetic disturbances were excited by primary alternating currents in a copper wire, 1,237 yards long, erected on poles along the top of the

cliff on the mainland. The radiant electro-magnetic energy was transformed into currents again in a secondary circuit, 610 yards long, laid along the island. The strength of these secondary induced currents complied almost exactly with calculations. The results attained, the apparatus used, the precautions taken to separate effects of induction from effects of conduction; the elimination of mere earth currents from electro-magnetic disturbances in air, will form the subject of a separate paper, for their proper consideration would be too tedious for an address. I allude to them now only to illustrate the existence of one of the greatest proofs of the truth of a theory, viz., the practical development and verification of a conclusion predicted from mere theoretical considerations.

The oscillatory character of the discharge of a Leyden jar, which was discovered by Henry in 1842, is an admirable proof of this mechanical theory. If two jars, precisely similar as regards capacity and circuit inertia, be placed near each other with their planes parallel, and one of them is charged and discharged, the other responds sympathetically, as do two similarly pitched tuning-forks when one is excited. Professor Oliver Lodge, who has made this field his own, has shown that by varying the capacity of the jars and the inertia of the circuit, oscillations can be produced to give any required rate of oscillation from one to 300 millions per second.

In a room or theatre, when these discharges are excited, it is a common thing to see sympathetic sparks upon the spangled walls, and among the metallic objects scattered about. The whole place is an electric field, which is violently disturbed at every spark, and everything which is "syntonised," as Oliver Lodge calls it, to the main discharge responds in this way.

It is impossible to account for these effects, which are all cases of transformed kinetic energy, except on the mechanical theory which I have advanced. We have a source of material disturbance, we have energy transmitted in ethereal waves, we have these waves transformed into disturbance again. Energy passes through its various stages by the motion of matter and the

action of the ether. Everything is accounted for and nothing is lost. Waste energy only means energy in the wrong place.

The advance of our knowledge in this branch of electrical development has been very much retarded by the phantasies of visionary mathematicians who monopolise the columns of our technical literature and fill the mind of the student with false conclusions. I have no sympathy with the pure mathematician who scorns the practical man, scoffs at his experience, directs the universe from his couch, and invents laws to suit his fads. The American, in his quaint, humorous way, said, "Theory "without practice is absolutely a worthless commodity; practice "without theory is worth about 15 dols. a week; but when both "are well combined in one man of sound judgment, the combina- "tion is worth up to 10,000 dols. a year."

My sympathies are with the earnest worker who asks Nature direct questions by sheer experimental prodding, who elicits from her stronghold the secrets of her prison-house, who presents gratuitously to the world the efforts of his labours in plain language, and who is willing modestly to follow in the footsteps of his great masters—Faraday, Maxwell, Kelvin, Von Helmholtz, and Rayleigh. Such men are to be found on both sides of the Atlantic. Our own proceedings chronicle the work of Hertz, Tesla, and Elihu Thomson.

I must not exclude the teacher, the much-abused Professor, who combines practice with theory, and who, in our technical institutions, is training up a body of youths destined to carry on the great work in the future. Amongst these able men there is no one who has earned a more richly deserved reputation than my predecessor in this chair (Professor Ayrton).

XII.—Conclusion.

Although my practical experience extends over 40 years, my personal reminiscences commence with the reign of the Queen, and are, therefore, coincident with the Victorian era. When we look around us and think of what we have now, it is well to think sometimes of what we had not in days gone by. I was bred in a Welsh seaport town which had no gas, there was not

a steam engine in the place, the mails came from London by coach, there was no railway in the country, photography was unknown, there was no daily paper, but an indifferent weekly journal was slightly supported. A letter to London cost 1s. The flint and steel and tinder-box were still in use, and the lucifer came one day as a wonder and a surprise. Artificial illumination was obtained amongst the lower classes from the rushlight and the tallow dip. The moulded candle came as a luxury to the upper classes. Snuffers were in use, and an oil lamp was a curiosity. Think of the luxuries we have now: of what science, and especially electricity, is daily doing for us. Progress is advancing with such rapid strides that, when the time comes for my son to chronicle to some institution of ethereal engineers his experience of 50 years' observation, the people of his day may look back with pity and amusement at the backwardness of the times of 1893. Even this address may be quoted as an illustration of the ignorance of the great Victorian period.

Cannot the electrical engineer claim some share in the progress of the world? Has he not robbed the thunder of its terror? Has he not annihilated space, bridged the ocean, and brought the Antipodes within the same fold as home? Has he not sent lightnings from heaven, so that they go and say unto us, "Here we are"? And are not all nations of the earth more amenable to reason and less subject to passion when they are brought within speaking distance of each other? Is it nothing to have brought health and comfort and cheerfulness into the house and security into the street? Is it not something to have rendered a first-class railway carriage, flying at the rate of 70 miles an hour, as safe as one's bed? He has probed the deep, unfathomed caves of ocean. He has extracted the precious metals from their stony bed. He warns us of approaching storms, and brings to our very hearths the tamed energy of the roaring torrent. Commerce, civilisation, politics, and life have all been benefited by his skill and invention, and history cannot fail to record the names of those, among whom are many members of this Institution, who, in this great Victorian era, have done so much to discharge one of the highest duties of man, the fulfilment of the functions of the true engineer.

APPENDIX.

In 1870 the speed of working between London and Dublin, attained by means of a relay and a Bain's chemical recorder, was 75 words per minute; it is now possible to work at a speed of 500, and the record is made in ink by the direct action of the line current.

In 1875 the maximum speed of simplex working on our land lines did not exceed 100 words per minute, while automatic duplex working was not practicable.

In 1877, although duplex key-working on our land lines was fairly good, the imperfect balancing appliances at our disposal made duplex working to Dublin very unsatisfactory, owing to the distortion of signals; and at this time Mr. B. Williams, of Haverfordwest, suggested the use of retarding resistances in connection with our balancing condensers, which greatly improved the duplex signals. In fact, we found that our enemies, apart from bad insulation, were resistance capacity and electro-magnetic inertia, and all our efforts were devoted to reducing their ill effects to a minimum.

In the same year repeaters were inserted in the Irish circuits *via* North Wales, and the speed of simplex working to Dublin was increased to 100 words per minute.

Until 1878 the Wheatstone duplex receivers could be employed only for key duplex working, but it was then found that by joining the coils of a high-resistance differentially wound receiver in multiple arc, a high simplex speed could be attained by the direct action of the line current on the receiver, and the Bain recorder and its relay were superseded.

The average speed of signalling was then limited for the first time by the mechanical construction of the automatic transmitters, and less importance was attached to very short electro-magnets to

reduce the effects of electro-magnetic inertia as a means of recording rapid signals.

Bichromate batteries were next introduced, and, owing to their low internal resistance, proved to be more suitable for high speeds.

The introduction of bichromate cells enabled us in 1879 to raise the simplex speed of the circuits from London to the Channel Islands from 90 to 120 words per minute, and soon afterwards duplex working on these wires was permanently established.

In 1880 it became necessary to alter the mechanical construction of our automatic transmitters and receivers to obtain a higher speed, and a rate of 200 words per minute was attained between London and the North of Scotland by dividing the circuits into three sections by two repeaters, one at Leeds and another at Edinburgh, instead of repeating automatically at Newcastle only.

The higher speeds involved special alterations in the perforators and in the mode of driving the receivers, recourse being had to weights instead of springs.

In 1881 shunted condensers were introduced to increase the speed of simplex reception, resulting in an increase on cable circuits from a normal 120 to fully 200 words per minute; and in 1883 fast-speed duplex repeaters were constructed, and the automatic transmitter was improved.

In 1884 a single signalling condenser was applied to cable duplex circuits, and the balancing condensers were divided into three adjustable portions, with the result that the duplex speed to Dublin was raised to 140 words per minute.

In 1885 double signalling condensers and a receiving condenser were attached to the cable duplex wires by Mr. A. Eden, and the speed consequently increased 25 per cent.

In 1886 a further improvement was effected in the transmitters, and this, in conjunction with a more exact knowledge of the application of the shunted condenser as a negative capacity, raised the speed of simplex working to 400 words per minute.

In 1889 another advance was made on all our cable circuits by changing the connections from differential to bridge duplex,

retaining the duplicate signalling condensers, and placing a shunt across the receiving condenser as in simplex high-speed working.

The result of this change was to further augment the duplex speed to the extent of 30 per cent.; the final outcome of all these improvements, in conjunction with the erection of copper wires, being that at the present time our practical simplex speed is 450, and our duplex speed between London and Dublin is at the rate of 300 words per minute, or a total of 600 words being signalled each minute on each of our Irish wires *via* North Wales.

Number and Mileage of Cables throughout the World in 1852.

Cable.	Number of Cables.	Length in Knots.	
		Cable.	Wire.
I. South Foreland—Sangatte (Calais) ...	1	22	88
II. Holyhead to Howth (Ireland)*	1	65	65
	2	87	153

* This cable failed upon the second day after laying.

Total Number and Mileage of Cables throughout the World in 1893.

Belonging to	Number of Cables.	Length in Knots.	
		Cable.	Wire.
I. Government Administrations	886	14,479	21,557
II. Private Companies	289	125,115	126,883
Grand totals	1,275	139,594	148,440

For details see following tables.

PRIVATE COMPANIES.

Number and Mileage of Cables throughout the World—1893.

	Company.	Number of Cables.	Length in Knots.	
			Cable.	Wire.
I.	Direct Spanish	4	708	708
II.	Spanish National	5	1,163	1,163
III.	India-Rubber, Gutta-Percha, and Telegraph Works Company	3	145	145
IV.	West African	12	3,015	3,015
V.	Black Sea...	1	346	346
VI.	Indo-European	2	15	50
VII.	Great Northern	24	6,948	7,176
VIII.	Eastern	76	25,376	25,383
IX.	Eastern and South African	9	6,645	6,645
X.	Eastern Extension, Australasia, and China	25	15,130	15,130
XI.	Anglo American	14	10,400	10,998
XII.	Direct United States	2	3,100	3,100
XIII.	Compagnie Française du Télégraphe de Paris à New York... ...	4	3,496	3,496
XIV.	Western Union	8	7,743	7,743
XV.	The Commercial Cable	6	6,908	7,748
XVI.	Halifax and Bermudas	1	850	850
XVII.	Brazilian Submarine	6	7,369	7,369
XVIII.	African Direct	7	2,746	2,746
XIX.	Cuba Submarine...	4	1,049	1,049
XX.	West India and Panama	22	4,557	4,557
XXI.	Société Française des Telegraphes Sous-marins	14	3,754	3,754
XXII.	Western and Brazilian	15	5,408	5,408
XXIII.	River Plate Telegraph	1	32	64
XXIV.	Mexican Telegraph	3	1,559	1,559
XXV.	Central and South American	12	4,898	4,898
XXVI.	West Coast of America	7	1,699	1,699
XXVII.	Compañia Telegraphico-Telefonica del Plata	1	28	56
XXVIII.	Compañia Telegraphico del Rio de la Plata	1	28	28
	Totals	289	125,115	126,883

GOVERNMENT ADMINISTRATIONS.

Number and Mileage of Cables throughout the World—1893.

	Administration	Number of Cables.	Length in Knots.	
			Cable.	Wire.
I.	Great Britain and Ireland	115	1,599	5,509
II.	Austria	31	105	113
III.	Belgium	2	54	278
IV.	Denmark	58	209	573
V.	France	54	3,450	3,910
VI	Germany	47	1,762	3,454
VII.	Greece	48	452	452
VIII.	Holland	20	60	81
IX.	Italy	39	1,068	1,133
X.	Norway	255	248	248
XI.	Russia (in Europe)	8	213	236
XII.	Spain	10	441	441
XIII.	Sweden	13	94	169
XIV.	Turkey (in Europe and Asia)	15	339	342
XV.	Senegal	1	3	3
XVI.	Russia (in Asia)	1	70	70
XVII.	Japan	31	215	277
XVIII.	Cochin China and Tonquin	2	795	795
XIX.	India (British)	83	1,927	1,927
XX.	India (Dutch)	4	483	483
XXI.	Australia (South)	5	50	50
XXII.	Queensland	13	162	162
XXIII.	New Caledonia	1	1	1
XXIV.	New Zealand	3	196	285
XXV.	America (British)	1	200	200
XXVI.	Bahama Islands	1	213	213
XXVII.	Brazil	22	35	47
XXVIII.	Argentine Republic	3	35	105
	Totals	886	14,479	21,557

THE WORLD'S CABLE FLEET AND THEIR USUAL STATIONS—1893.

Owners, &c.	Name.	Tonnage (Displacement).	H.P. (Indicated).	Station.
British Government	Monarch ..	2,173	1,040	Woolwich.
" "	The Lady Carmichael	750	450	Dover.
		Gross Tonnage	H.P. Nominal	
Indian Government	Patrick Stewart	1,115	130	Karachi.
Canadian "	Newfield ...	785	90	Halifax, N.S.
French "	Ampère ...	304	70	Brest.
" "	Charente ...	548	120	La Seyne.
Chinese "	Fee Cheu ...	1,034	150	Formosa.
Private Companies.				
Anglo-American...	Minia	1,986	250	Halifax, N.S.
Central and South American ..	Relay	—	—	Callao.
Commercial Cable	Mackay-Bennett	1,718	300	Halifax, N.S
Cie Française du Telégraphe de Paris à New York	Pouyer-Quertier	1,385	160	Havre.
Eastern...	Amber	1,034	250	Gibraltar.
"	Electra	1,210	200	Suez.
"	John Pender	1,213	98	London.
"	Chiltern ...	1,372	200	Aden.
"	Mirror	1,545	250	Alexandria.
Eastern and South African ...	Gt. Northern	1,352	130	Zanzibar.
Eastern Extension	Recorder	1,201	200	Singapore.
" "	Sherard Osborn	1,429	200	"
Great Northern	H. C. Œrsted	749	120	Copenhagen.
" "	Store Nordiske	832	120	Shanghai.
India-Rubber Co. (Silvertown)	Buccaneer ...	785	180	Silvertown.
" " "	Dacia	1,856	170	"
" " "	International	1,381	110	"
" " "	Silvertown ...	4,935	400	"
Pirelli & Co.	Citta di Milano	1,220	220	Spezzia.
Siemens Bros. & Co.	Faraday .	4,917	500	London.
Société Genl. des Téléphones ...	Francois Arago	3,191	300	Calais.
Telegraph Construction and Maintenance	Britannia ...	1,525	180	London.
" " " ...	Calabria ...	3,321	220	"
" " " ...	Scotia	4,667	550	"
" " " ...	Seine	3,553	341	"
Western and Brazilian	Norseman ..	1,372	200	Pernambuco
" "	Viking	436	60	Monte Video.
West India and Panama ...	Duchess of Marlborough	402	80	West Indies.
" " "	Grappler ..	868	100	" "
West Coast of America	Retriever ...	642	95	Callao.

The cable fleet of the world consists, therefore, of 37 ships, 7 of which belong to Government administrations.

Mr. SPAGNOLETTI: I rise with a very great deal of pleasure to perform a very pleasing duty. The proposition I have to make I am sure will only require to be uttered to receive from your hands the reception it deserves. It is—"That the cordial thanks "of the Institution are due to the President for the admirable "and highly interesting Address just delivered by him, and that "he be requested to permit its publication in the Journal of the "Institution." As Mr. Preece has told us, his long life of labour of 40 years qualifies him eminently for giving us an address on the history of the past up to the present, and also of the numerous applications to which electricity has been applied. He told us—I do not think we should have known it if he had not—that he is beginning to feel the evidences of age growing upon him; but we may congratulate him upon his appearance here this evening, and to hope that for many years he will enjoy good health and continue in active work.

Sir HENRY MANCE: As an old telegraphist, I have much pleasure in seconding this proposition for a vote of thanks. I do not wish for one moment to underrate the great work our President has done in connection with the application of electricity to power and light; but we old telegraphists and telegraph engineers are accustomed to regard Mr. Preece as the head of the telegraph profession, and I am sure this Address will be read with very great pleasure by our members abroad, the majority of whom are telegraph men. There was one point in his Address which struck me with dismay, and that is the gradual increase of the teredo in the neighbourhood of our shores. This fact was brought home to me to-day by specimens of cable recently attacked by that insect, or mollusc, and it should teach us—what Mr. Preece told us many years ago—that we should not only survey the bottom of the sea for rocks and shoals, but should also examine it near the shores to see if it is infested by that pest, which has damaged hundreds of thousands of pounds' worth of cable. I beg to second the motion for a vote of thanks to our President for his comprehensive, instructive, and most eloquent Address.

The motion was carried with enthusiasm.

The PRESIDENT: By far the happiest man in this room to-night is the President. He has succeeded in securing your approbation, and he has delivered himself of a nightmare. I thank you deeply for the kindness with which you have received my Address, and also my old friends, the proposer and seconder of the vote of thanks, for the kind terms which they respectively used.

I now have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Vincent Sydney Allpress.

Associates:

Albert Anns.
Matthew Henry Barber.
John Richard Bradford.
H. Davis.
William Douglas.
Frederic Theodor Eggers.
Russell Forrester Fergusson.
Alexander William Frayne.
Edward Leader Hanna.
Charles Edward Jones.
Francis Lydall.
M. R. Margesson.
W. C. Martin.
Michael Charles Meaby.
Charles Wesley Sargeant.
John Sinnott.
Charles A. Stevenson, M. Inst. C.E., F.R.S.E.
Philip Ibotson Unwin.
Alfred Watson.
George A. P. Weymouth.
F. A. S. Wormull.

Students:

Alec Alfred Beadle.
Walter Eynon.
Gilbert Holt Green.
Douglas Kerr Hall.
George Kemp.
William Murray Morrison.
Herbert Bryan Poynder.

The meeting then adjourned.

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS.—No. 104.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII. 1893. No. 104.

The Two Hundred and Forty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 9th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The Minutes of the Ordinary General Meeting held on January 26th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Herbert James Bubb.	W. Elsdon Dew.
Walter Claypole.	Thomas Arthur Rose.

Mr. J. E. Stewart and Mr. A. G. Seaman were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from Dr. Hopkinson and Professor S. P. Thompson, Members, to whom the thanks of the meeting were accorded.

The PRESIDENT: Gentlemen,—It is my very pleasing duty to have to announce to you that at the Council meeting to-night we received from Sir David Salomons another very handsome donation, in the shape of £500 New South Wales 3½ per cent. Stock, to be added to the Salomons Scholarship Fund; and at the same time he presented us with £20 for the Benevolent Fund. I am quite sure I am only anticipating the feelings of the whole meeting when I move that we accord to Sir David Salomons a hearty vote of thanks for his munificence.

The motion was carried enthusiastically.

The PRESIDENT: Before calling on Professor Fleming to favour us with his reply to the discussion which his paper has given rise to, I will ask Professor Ayrton to give us some remarks which I understand he would like to make upon the subject.

Professor Ayrton

Professor W. E. AYRTON: It was only a few minutes ago that I was informed by the President that I had to speak this evening on Dr. Fleming's paper. When that paper was read, I made a number of marginal notes on the proof of good things which I intended to say; but I did not bring that proof this evening, as I had no idea that the discussion on this paper was to be continued. However, in obedience to the President's request, I will say something, even if it be not the something which I had intended to say.

Dr. Fleming throughout his paper has used the abbreviations "ammeter" and "P.D.," and, by thus winning the heart of their proposer, has almost disarmed criticism on my part. By the bye, I noticed the other day that the expression "P.D." was called slang in the *Electrician*. I suppose "Salisbury Court, E.C.," would be also slang. I thought of writing to say so, but remembering that my signature, "W. E. Ayrton," would also be slang, I refrained from doing so.

The use of initial letters as abbreviations such as "A.D." for *anno domini*, "P.S." for *post scriptum*, &c., are, of course, not slang. Neither, therefore, is the employment of "P.D." for potential difference. So that, emboldened by the success that my former suggested abbreviations have met with, I will now propose another, to enable the objectionable expressions "effective current" and "virtual current" to be dispensed with.

Professor Ayrton.

What should be called the effective current clearly should depend on what is the effect you desire to produce. A direct current, such as that produced by a Brush or a Thomson-Houston dynamo, can, of course, be used for electro-deposition although the current periodically varies in strength. But the metal deposited in a given time in an electrolytic bath depends on the mean current, as measured by a D'Arsonval galvanometer, and not on the square root of the mean square, as measured by a dynamometer, or by an ammeter with a soft iron needle. In this case, then, the mean current ought to be called the effective current. But the same current can be employed to make a glow lamp incandesce, and in that case the effective current is the square root of the mean square. And, lastly, if a condenser be placed as a shunt to the machine there will be a small alternating current through the condenser, the effective P.D. for producing which is neither the mean P.D. at the terminals of the dynamo, nor the square root of the mean square, but the rate of variation of the terminal P.D.

Again, "virtual current" means a current, I presume, virtually equivalent to something else, and is virtually as bad and no more effective than the expression "effective current."

With all due deference to the Paris Congress, I, therefore, propose that the words "effective" and "virtual" should be dropped, and that for abbreviation the mean current should be called the "M. current," the mean square the "M.S. current," and the square root of the mean square the "R.M.S. current." A D'Arsonval galvanometer then reads the "M. current." As regards the "M.S. current" and the "R.M.S. current," I have had two scales attached to Siemens dynamometers in my laboratory. With the outer, the "M.S. current" can be at once read off in amperes squared, and with the inner the "R.M.S. current" in amperes. And I may mention that in drawing these scales attention has been given to the variation of the constant in different parts of the scale of a Siemens dynamometer, to which Professor Perry and I drew attention some years ago at the Physical Society, and which arises from a distortion of the spring, which occurs even for so small a deflection as 180°.

Professor Ayrton.

While on the subject of dynamometers, I may mention that I was very glad to hear that Dr. Fleming had found so useful the method that Dr. Sumpner and I suggested for diminishing the self-induction error with a wattmeter. This method, which was given in one of the few of our Physical Society's papers that was written, and therefore published,* consisted in replacing the many-convolution fine-wire coil of a wattmeter, and its accompanying stationary non-inductive high resistance, by a few turns of thick wire and an auxiliary low resistance of thick wire—by using, in fact, an ordinary dynamometer as a wattmeter with a low-resistance pressure coil. No doubt in this way you waste a great deal more power in making your measurement; but if you are anxious to obtain a perfectly accurate result, the waste of power in the wattmeter itself is quite unimportant compared with the accuracy obtainable. This waste of power does not enter into the measurement; it merely means that the dynamo has to do a little more work.

As to the construction of electrostatic voltmeters, I have already explained at a meeting of the Institution of Civil Engineers why I think that the employment of flat needles is a mistake. Mr. Mather and I propose shortly to offer a paper to this Society on the theory of the design of electrostatic measuring instruments, and I will, therefore, now do no more than say that while Professor Fleming has only succeeded in reading to an accuracy of ½ per cent. when using a reflecting zero Swinburne electrostatic voltmeter, we find that without a mirror, and without the necessity of any zero adjustment, it is comparatively easy to read to 1-10th per cent. on the working part of the scale with a simple inspectional electrostatic voltmeter properly constructed.

On the last occasion fun was poked at Professors, and at their Hampton-Court-Mazy ways of testing transformers; but had Mr. Swinburne condescended to use the three-voltmeter method for measuring power, he would neither have sent out transformers with a far greater iron loss than he stated that they

* "Alternate Current and Potential Difference Analogies in the Methods of Measuring Power."—*Phil. Mag.*, p. 212, August, 1891.

possessed, nor would he have constructed, and used, a wattmeter which read wrongly when used to test "hedgehog" transformers, in consequence, as Dr. Sumpner has proved, of Foucault currents being generated in the brass supporting rods of the wattmeter coils. Professor Ayrton.

When publishing the three-voltmeter method, it was not the intention of Dr. Sumpner and myself that it should always be employed whenever the power given by an alternating current to a circuit had to be measured; but we regarded its use as we regard the use of a Kelvin balance for measuring current. I should, for example, not think of measuring every current commercially by means of a Kelvin balance—it would be a too troublesome and lengthy a process; but, if I had doubts about the accuracy of the readings of an alternate-current ammeter, I should check them by comparing the ammeter with the Kelvin balance. So, in the same way, before pronouncing the readings of a wattmeter correct, and before trusting to any important results that this wattmeter might appear to bring out when employed to measure a very inductive load, I should check the wattmeter by means of our three-voltmeter method.

And although Dr. Fleming has made a large number of experiments, some of which show what, it is to be observed, our formula published on page 609 of his paper shows equally well, without the necessity of making a series of experiments—viz., that when the lag is great an error in reading the voltmeter produces a serious error in the measurement of power by the three-voltmeter method—he has, as he has himself written to me, "relied, as a last resource, on the carefully performed three-"voltmeter method as a Court of Appeal."

The experiments that Dr. Fleming has so carefully carried out have confirmed the results of the tests that were published last summer by Dr. Sumpner and myself at the meeting of the British Association in Edinburgh. Then we stated that our tests "show that neither at no load nor at full load is there "any great difference apparently between the power wasted in "a 'hedgehog' and in a Mordey type of transformer of the "same output." Tables of results of tests of the closed magnetic

Professor Ayrton.

circuit type of transformer at different frequencies were given in that communication; and in a paper published in the *Electrician* for November 25th of last year we gave the results of further tests of a "hedgehog" and of a Mordey type of transformer which showed that the iron and copper losses in the former were each respectively greater than the iron and copper losses in the latter.

Mr. Swinburne has therefore undoubtedly made a mistake; but you know the adage, "He who never made a mistake never "made anything else." Now I venture to think Mr. Swinburne has done so much to advance our knowledge of alternate-current working that he is almost justified in making a mistake.

Dr. Fleming has coupled the methods of testing transformers employed by Dr. Hopkinson and by Dr. Sumpner and myself. They have, no doubt, this in common: that with both methods the loss of power is directly measured instead of being deduced from the difference of two nearly equal quantities—the power put into the transformer, and the power put out. But there is this most important difference—that, while Dr. Hopkinson's analytical method can only be employed in a laboratory with fairly delicate apparatus, the plan of testing employed at the Central Institution is essentially a workshop one, as nothing is required beyond ordinary workshop measuring. It is important to notice that, because people might be led to think, from the way in which the method is referred to in Dr. Fleming's paper, that it was very difficult to use the method we have been employing at the Central Institution. It requires, undoubtedly, two transformers, but those two transformers need not be even of the same type. Indeed, all the tests that we made on the "hedgehog" transformer, using this method, were not carried out with two "hedgehog" transformers, but were made with a "hedgehog" transformer combined with a Mordey type of closed magnetic circuit transformer, showing that the method can be employed with success even when the two transformers differ so radically in type as do an open magnetic circuit transformer and a closed magnetic circuit transformer.

Our method further not only saves power and enables a large

transformer to be tested with a small alternator, but it gives the magnitudes of the iron and of the copper losses *directly* and *separately* by the respective readings of two wattmeters without any calculation whatever. That Dr. Sumpner's method is essentially a workshop method is best proved by Mr. Mordey having already adopted it for regular use in the works of the Brush Electrical Engineering Company. Professor Ayrton.

Mr. W. B. SAYERS [*communicated*]: The following description of an experimental demonstration of the crowding towards the surface of iron core of alternating magnetic flux will, I think, not be out of place in the discussion on Dr. Fleming's paper. Mr. Sayers.

An iron ring of 1 inch square section, 2 inches internal diameter, and 4 inches external, was bored in two places with four holes, crossing each other, so as to form passages beneath the outer layer of metal for an exploring wire, in the manner shown in the figures below.

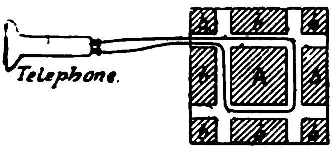


FIG. 1.

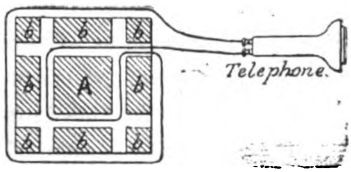


FIG. 2.

Sections through iron ring showing holes for exploring wire.

The holes are drilled so as to make the sum of the areas *b* (Fig. 1) approximately equal to the area of the central portion, A. Wires were then threaded through the holes—in one case as shown in Fig. 1, and in the other case as shown in Fig. 2; each circuit being connected with a telephone. It will be seen that the flux through the area A will alone affect the telephone in one case (Fig. 1), and that flowing through all the areas *b* (Fig. 2) in the other case.

The ring was wound with about 300 turns of wire, and a current of about 0·6 ampere (mean) 80 $\sim$ sent through. The telephone in case 1 was almost, if not quite, silent, while in case 2 the note corresponding to the period was clearly audible.

Further experiments are contemplated on the same principle, with a view, if possible, of getting some quantitive results,

Mr. Sayers

especially when plates such as are used in transformers transmit the induction, instead of the square iron—in which, of course, the effect is comparatively very great.

From some calculations of a somewhat rough-and-ready nature, I estimate that an induction which reaches an average maximum density of 5,000, alternating at a rate of 100 ∿ per second and flowing through a plate 1 mm. thick, will be fully three times as dense near the surface of the plate as it is at the centre. I do not know how this compares with Mr. Evershed's figures, or those of Professor J. J. Thompson, which I have not seen; but, if I am correct, the effect appears to have more importance than has been attached to it. Rise of temperature will reduce eddy-currents in consequence of increase of resistance of iron, and so allow a more even distribution of magnetic flux and consequent lower *maximum* density.

Thus the total hysteresis loss will be less with higher temperature. Is it possible that this may explain Mr. Mordey's observed reduction of hysteresis loss with *increased load?*

In the case of transformer cores in which the plates overlap considerably, as in the Mordey transformer (see A, Fig. 3), the action of the eddy-currents will be to cause the induction to pass from one plate to the other, chiefly at the edges and corners, instead of evenly over the opposed surfaces. If this were not so, the loss from eddy-currents *in the overlapping parts of the plates* would, I take it, be very considerable, and quite independent of the thickness of the plates.

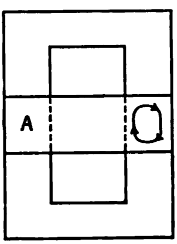


Fig. 3.

Dr. Fleming.

Dr. Fleming: It often happens that at the end of a long discussion, by the time the author rises to reply, he finds that many of those who have spoken have answered one another's difficulties, criticisms, or objections, and that little remains for him to do but to pair off these speakers one against the other, and to point out where their remarks and arguments meet one another.

Although the general accuracy of the figures and statements in the paper I have had the honour of presenting to you has not

been substantially impugned, nevertheless, I am bound, in this instance, to go rather farther in reply than the above limits, and to answer with some care and detail the remarks of several speakers who have taken part in the debate. I therefore gladly availed myself of the permission of the Council to make this reply *vivâ voce* rather than hand it in in writing, because I think I shall in this way best be able to bring home to your minds facts and arguments which may be urged against views that have been put forward by several who have taken part in discussing my paper. As is most fitting, I take these speakers in the order in which they have spoken. Dr. Fleming.

Mr. Swinburne may be considered to be the chief defendant, because a good deal of my paper referred to his work, and, although agreeing with me in some things, he, naturally, differed very much in others.

His first remark concerned his electrostatic voltmeter; he said his firm had considered the question of employing the oil-damping arrangement I suggested, but they preferred the electro-magnetic damping, and that my failure to work the voltmeter was due to my not having employed sufficient current for the electro-magnets.

Mr. Swinburne's speech was made on December 1st, and reported in the *Electrician* of December 16th, and, being marked by an asterisk as having been corrected by the author, may perhaps be taken to represent correctly what he stated when he took part in the discussion on the 1st of December. On referring to the *Electrician* for December 2nd, you will find a short article, having all the appearance of an inspired communication, in which the following statement occurs in speaking of the Swinburne electrostatic voltmeter. It says:

"The electro-magnetic damping arrangement has been done "away with, and an adjustable oil damper substituted, which "enables any degree of damping desired to be obtained."

My paper was read on November the 24th, so that you will see that, practically, it comes to this: Mr. Swinburne adopts and utilises my suggestions about oil damping on December the 2nd, and he repudiates them again on December 16th. I must leave

Dr. Fleming you and him to reconcile, as far as possible, these contradictory opinions.

With regard to the use of mica for preventing discharges from the needle to the case, it is quite true that we used these voltmeters for higher pressures than those for which they were originally designed; but we deserve every credit for thus converting an instrument arranged by its maker for 1,200 volts, into one which can be used up to 3,000. The only objection which Mr. Swinburne is able to urge against the use of mica for clothing the "needle" is, that it renders it impossible to calibrate the instrument from standard cells. The calibration of the instrument up to 2,000 volts, with standard cells, is an operation that requires the very greatest care, in order to avoid serious errors, and I cannot conceive how anybody could deliberately prefer this method to the exceedingly simple and effective one, which I described to you, employing a non-inductive divided resistance and a voltmeter working up to 100 volts, which can be standardised, if need be, by direct reference to a Clark cell. I have not the slightest doubt that, sooner or later, Mr. Swinburne will adopt the use of the mica, or, if he does not, all those who purchase voltmeters from him will adopt it for themselves, and find the greatest convenience from so doing.

Coming next to the much-discussed wattmeters: There is no need, I think, to thrash a dead horse. It is admitted that the first wattmeter Mr. Swinburne sent was not correct, but that the second one was, and the only difference of opinion which exists is as to the causes of the error in the first-sent instrument. Mr. Swinburne still thinks, judging from his printed remarks in the report of the discussion, that eddy-currents in the case have something to do with this error. It is perfectly clear that he failed to notice in my paper, on page 674, a table of experiments in which this particular wattmeter was used with the case *on* and with the case *off*, and in which it is shown that the removal of the brass case made little or no difference in the erroneous low readings of the wattmeter; hence it is perfectly evident that the substantial cause of the error is not eddy-currents in the case.

I think that the interesting figures which Dr. Sumpner has

obtained from his own experiments, and from the analysis of the figures in my paper, sufficiently show that, at any rate, in this wattmeter practically the whole of the error can be accounted for by local currents, which are set up in the brass plates which carry the series coil; and that, assuming this as the cause, the vagaries of the instrument can be accounted for by this supposition alone. Dr. Fleming.

However, the practical result is that in an alternating-current wattmeter it is necessary, as I say in the paper, that no metal plates or supports of any kind should be placed near the movable coil.

In the next place, Mr. Swinburne objects to my term "power-"factor." It is very difficult to know how to please him in the matter of terms: he objects still more to the use of "cosines" and "angles of lag;" and I cannot understand what valid objection there can be to the employment of such a simple term to express a ratio which is very frequently required to be considered, namely, the ratio between the real and apparent power given up to the circuit by alternating currents. Although he protests against the use of the term, in the same breath he claims to have been the first to employ it.

We now come to the real question at issue, namely, the question of open *versus* closed magnetic circuit transformers. Mr. Swinburne says that I have not settled it. No matter ever is settled finally and for ever in which invention is still possible, and we trust and hope that Mr. Swinburne will long continue to employ his fertile ingenuity in the improvement of transformers; but what I *do* claim to have settled, and what I shall demonstrate to you I *have* settled, is the fact that, up to date, the best open-circuit transformer that can be produced is not electrically better than the closed-circuit transformer of equal output. Now, when we say "not better" or "better than" in speaking of two transformers, we must consider carefully what it is we really mean. We cannot assert anything of this kind merely in the abstract, and it is perfectly useless to assert it with reference merely to instantaneous efficiencies. In order to test it we must assume certain conditions of working—in other words, assume a certain load diagram—and then, on that load diagram, compare the

Dr. Fleming. all-day efficiencies of two transformers of equal nominal output, working under the same conditions. The figures in my paper will enable you to do this for yourselves, but, in order to bring home the facts, I have prepared a series of tables for the purpose of such comparison. In the first place, I have compared together the all-day efficiency of a 6,000-watt "hedgehog" and a 6,000-watt "Mordey" transformer, when working on a 10 per cent. load-factor. It does not matter for our present purpose whether the 10 per cent. load-factor is larger or smaller than is met with in actual practice—you may take any assumed load diagram—but for the purposes of this argument I have imagined a load diagram of the kind stated in Table I., and on this load diagram I have compared the performance of these two equal transformers by making an energy balance-sheet for each transformer for the 24 hours; and you will see from Tables I. and II. that for the open-circuit transformer the all-day efficiency on this diagram is 74·2 per cent., and for the closed-circuit transformer it is 74·3 per cent.

Table I.—All-Day Efficiency of 6,000-Watt "Hedgehog" Transformer on a 10 per cent. Load-Factor.

Let the load diagram be taken as equivalent to

$$24 = \begin{cases} 11 \text{ hours working at no secondary load,} \\ 5 \quad ,, \quad ,, \quad ,, \text{ 1-10th of full load,} \\ 4 \quad ,, \quad ,, \quad ,, \text{ 1-8th} \quad ,, \; ,, \; ,, \\ 3 \quad ,, \quad ,, \quad ,, \text{ 1-4th} \quad ,, \; ,, \; ,, \\ 1 \quad ,, \quad ,, \quad ,, \text{ 3-4ths} \quad ,, \; ,, \; ,, \end{cases}$$

which is equivalent to 2·5 hours at full load, or to a load-factor of *very* nearly 10 per cent.

The energy balance-sheet for the transformer on this load, as derived from tables of observations, is as follows:—

Energy in Watt-Hours given to Primary Circuit.	Energy in Watt-Hours taken from Secondary Circuit.
11 × 151 = 1,661	11 × 0 = 0
5 × 767 = 3,835	5 × 600 = 3,000
4 × 919 = 3,676	4 × 750 = 3,000
3 × 1,674 = 5,022	3 × 1,500 = 4,500
1 × 4,680 = 4,680	1 × 4,500 = 4,500
Total = 18·874 B.T.U.	Total = 15·000 B.T.U.

Dr. Fleming.

Energy loss = difference = 3·874 units in 24 hours.

Iron all-day loss = 24 × 151 = 3·624 units.

Copper all-day loss = 0·250 „

Total loss = 3·874 „

$$\left.\begin{array}{r}\text{All-day efficiency on this}\\ \text{10 per cent. load-factor}\end{array}\right\} = \frac{15\cdot000}{18\cdot874} = 74\cdot2 \text{ per cent.}$$

Table II.—All-Day Efficiency of 6,000-*Watt Mordey Transformer on the same above* 10 *per cent. Load-Factor.*

The energy balance-sheet for the transformer on this load, as derived from the tables, is as follows:—

Energy in Watt-Hours given to Primary Circuit.	Energy in Watt-Hours taken from Secondary Circuit.
11 × 148 = 1,628	11 × 0 = 0
5 × 768 = 3,840	5 × 600 = 3,000
4 × 911 = 3,644	4 × 750 = 3,000
3 × 1,668 = 5,004	3 × 1,500 = 4,500
1 × 4,720 = 4,720	1 × 4,500 = 4,500
Total = 18·836 B.T.U.	Total = 15·000 B.T.U.

Energy loss = difference = 3·836 units in 24 hours.

Iron all-day loss = 24 × 148 = 3·552 units.

Copper all-day loss = 0·284 „

Total loss = 3·836 „

$$\left.\begin{array}{r}\text{All-day efficiency on this}\\ \text{10 per cent. load-factor}\end{array}\right\} = \frac{15\cdot000}{18\cdot836} = 74\cdot3 \text{ per cent.}$$

The total 24-hour loss for the open-circuit transformer is 3·874 units, and the same loss for the closed-circuit transformer is 3·836. You may vary these diagrams as you please; you may take any load diagram you can imagine, or that you think is likely to occur in real practice, and, taking the figures from the paper, you will find that, under all the ordinary conditions of work, the 6,000-watt closed-circuit transformer gives a slightly higher all-day efficiency than the 6,000 open-circuit transformer, and that the accuracy of these figures cannot be impugned.

It is interesting to go a little farther than this and to see how far these internal losses in the transformers are supplemented by

Dr. Fleming. losses in the primary main when the transformers are working on the same load diagram. I have, therefore, assumed a case, which is quite capable of being realised in practice—namely, a primary main a mile long, consisting of a 7/16 cable in each branch, and four 6,000-watt transformers placed at the end of that primary line.

Table III.

C^2R Losses in the Primary Cable on the assumption that four 6,000-watt transformers are placed at the end of one mile run of double primary cable, each single cable having a copper section equal to 7/16 and a total go and return resistance of 4 ohms.

Primary pressure at transformer terminals = 2,400 volts. Total volts lost on primary cable at full load = 40, or under 2 per cent. Load diagram as in Tables I and II.

A.—Four 6,000-Watt "Hedgehog" Transformers.

Energy Loss in Cable in Watt-Hours.

$$11 \times (1{\cdot}194 \times 4)^2 \times 4 = 1{,}003$$
$$5 \times (1{\cdot}228 \times 4)^2 \times 4 = 482$$
$$4 \times (1{\cdot}270 \times 4)^2 \times 4 = 413$$
$$3 \times (1{\cdot}410 \times 4)^2 \times 4 = 381$$
$$1 \times (2{\cdot}436 \times 4)^2 \times 4 = 380$$

Total = 2·659 B.T.U.

B.—Four 6,000-Watt Mordey Transformers.

Energy Loss in Cable in Watt-Hours.

$$11 \times (0{\cdot}076 \times 4)^2 \times 4 = 41$$
$$5 \times (0{\cdot}323 \times 4)^2 \times 4 = 33$$
$$4 \times (0{\cdot}387 \times 4)^2 \times 4 = 38$$
$$3 \times (0{\cdot}692 \times 4)^2 \times 4 = 92$$
$$1 \times (1{\cdot}980 \times 4)^2 \times 4 = 250$$

Total = 0·454 B.T.U.

In Table III. you will see calculated out the 24 hours' loss in the primary cable for each of these cases. In the case of the four "hedgehog" transformers, the 24 hours' loss in the line will be 2·659 units. For the four open-circuit transformers (Mordey) the 24 hours' loss in the primary line will be 0·454 of a unit.

If the total losses in transformers and cables are taken for the 24 hours in the two cases, we find that, while the four "hedgehogs" and cable dissipate 18·155 units, the four Mordey transformers of the same size and the cable dissipate only 15·798 units, the secondary circuit delivery in both cases being 60 units. In addition to the cable losses, there would also be the relative losses in the dynamo armature; but I find, on calculation, that, owing to the low resistance of modern armatures, the armature loss would really be quite a negligible matter, even in the case of the "hedgehog," as compared with the losses in the primary cables. Hence we see, from Table III., that, in order to deliver on such a load diagram 60 units in 24 hours at a distance of one mile, by four 6-kilowatt Mordey transformers, we have to dissipate 15·7 units; to do it by four 6-kilowatt "hedgehogs" would require 18·2 units. If you will be good enough to look carefully at this table, you will then be able to ask yourselves, and also answer the question, whether there is any foundation in fact whatever for the statement so frequently made that we can afford to throw away our closed-circuit transformers, and replace them by the much more economical open magnetised circuit transformers. The difference between the total energy losses in the two cases in the 24 hours would amount to about 2·36 units, and in 365 days this would signify a total of 860 units, which at 3d. per unit is £10. Dr. Fleming.

When the open-circuit transformer first came before us, and after it began to be realised that the large magnetising current, in the "hedgehog," at any rate, meant a considerably greater loss in the primary cables than would be the case for the closed magnetic circuit transformer of the same size, Mr. Swinburne proposed to remedy this fault by employing a condenser to supply the necessary magnetic current of the transformer; and he has, I believe, spent a considerable amount of time in developing a condenser for this purpose, capable of being used on high-tension circuits.

It is a very important matter to ascertain whether the proposed remedy is really a remedy. It is very easy to make the assumption of theoretical perfection in condensers; but none of

Dr. Fleming. the condensers which have been submitted to me for test have proved themselves to be without dielectric hysteresis, and it appears that this may amount to a very considerable quantity of energy lost in 24 hours. For instance, a condenser was submitted to me which passed, at a pressure of 2,400 volts, current of about 4 amperes: such a condenser would probably be used to supply current for four 6,000-watt "hedgehog" transformers. This condenser absorbed 120 watts. Let us, therefore, suppose that such a condenser is placed at the dynamo end of the primary line, to supply the four transformers at the other end with magnetising current. I very much doubt whether a condenser made of paper (as these are made) can be made to have a power-factor of less than 0·01, and would be found to take up quite about 100 watts in power; and, as I say, the one submitted to me took up a great deal more. If this is the case, it is, then, obvious that in the 24 hours such a condenser would dissipate 2·4 B.T.U. of electric energy.

The object of putting a condenser there at all is to save the loss in the line. If you look for a moment at Table III., you will see that the loss in the primary line in the case of the four "hedgehog" transformers is 2·659 B.T.U., while in the case of the four closed-circuit transformers it is only 0·454 B.T.U.; the difference is therefore 2·205, and this is the difference which has to be supplied in order to reduce the losses in the line to the value which they would have with good closed-circuit transformers at the other end. There would also be a slight reduction in the case of the copper losses of the transformer; but as these together only amount to a quarter of a unit in the 24 hours, we see by a comparison of these figures that if the condenser dissipates 2·4 B.T.U., and the utmost amount that it can save on this 10 per cent. load diagram is something like 2·3 B.T.U., or rather less than this, the addition of the condenser to the line is merely robbing Peter to pay Paul—it puts on, on the whole, rather more than it takes off from the energy balance-sheet—hence, although it is very easy to talk about adding condensers to save magnetising energy and copper losses in the line, unless an actual condenser can be produced which is absolutely free from

dielectric hysteresis, it merely changes the place of the waste of energy; and I think most engineers would prefer to have the copper loss in the line where they do know it is safe, to the energy loss in a condenser which may give rise to undesirable effects. It is obvious that at less load-factors the condenser is a source of waste of power. Dr. Fleming

It is perfectly clear to my mind that a condenser, therefore, does not necessarily effect the saving which Mr. Swinburne thinks it does, and that he would in many cases do better without than with it, especially when working on low load-factors.

I think I need not labour this point any more. You have the figures in my paper before you, and it will be possible for you to work out any number of similar cases for yourselves, and to ascertain exactly what are the actual diurnal losses on any given load diagram which you please to suppose.

With regard to methods of measuring the drop of the secondary circuit in transformers, Mr. Swinburne appears to have misunderstood me altogether. It is perfectly clear that if we subtract the total measured secondary drop from the drop which we can account for by the resistance of the primary and secondary circuit, the remainder which is not thus accounted for must be due, in some way or other, to what we call generally "magnetic "leakage." Whether that will be more or less if the secondary circuit is inductive does not matter. All my experiments were made with secondary circuits composed of lamps, because those are the chief practical conditions with which we have at present to do.

Mr. Swinburne went on to give us an explanation of the reason why he thinks this present form of transformer is imperfect. He repudiates and rejects my explanation (that it is due to eddy-current losses in the copper), but, other than the mere contradiction, he did not furnish the members of this Institution with any figures to disprove my hypothesis or to support his own. A serious consideration of Professor Fitzgerald's remarks may, however, serve to alter his views. He went on to point out that he considered the real source of the present non-superiority in practice of the present "hedgehog" to its closed-circuit rival was

Dr. Fleming. to be found in the fact that the core had been squeezed too tightly together in winding the transformer, and that thereby increased eddy-current loss had been created in the iron. This *may* be an explanation of the case, but, at any rate, he has furnished us with no figures to support his statement. Nothing would be easier than to take a bobbin of wire and insert into it a very tightly squeezed bundle of iron wires, and also the same mass of iron wires not squeezed together, and test by the wattmeter whether the losses in the two cases were the same. On this point we may have to suspend our judgment until someone takes the trouble to verify or disprove this statement; but meanwhile I venture to suggest to Mr. Swinburne that there is one drastic remedy that he may apply without hesitation, and that is, to return to the kind of "hedgehog" transformer that was first devised by Mr. Cromwell Varley in 1856. Mr. Swinburne's "hedgehog" transformer has its "hedgehog" ends very little projecting beyond the coils. Mr. Varley made a transformer in 1856 in which he left the ends of the wires at both ends long enough to be folded back and meet together round the body of the transformer. It would be very easy to arrange matters so as to secure sufficient ventilation, and by returning to Mr. Varley's type of transformer and making a closed magnetic circuit Mr. Swinburne would get rid at one blow of all his difficulties, and arrive without trouble at a thoroughly satisfactory and economical alternating transformer.

Professor Forbes then took up the discussion, and made some very valuable remarks upon the importance of designing transformers specially for their work. I fear we have been too much urged to admire mere high efficiency at full load without regard to the nature of the work to be done. This is very much like recommending a horse because he is 16 hands high, without regard to whether he is intended to drag a dray or carry a light weight in the hunting field. The advent of large schemes for power-transmission will show the necessity of designing transformers specially with regard to their character of work, and of adopting probably a much lower frequency than is now employed in the majority of transformers used for electric lighting.

In connection with the Westinghouse transformer, no one seems to have called further attention to the fact which I pointed out—that the core of the Westinghouse transformer tested by me was made of mild steel. When Mr. Tesla was over in England, he told me that, by much experiment, the Westinghouse Company had adopted this material for the cores of transformers. Practically, what they do is probably to prepare a type of pure Bessemer steel, which is as nearly as possible free from carbon and manganese. I know the great difficulties that manufacturers of transformers have to contend with in procuring identical qualities of iron on all occasions; and it seems to me that the only way out of this condition of things will be for some enterprising persons to turn their attention seriously to the production of electrolytic iron of definite chemical purity, and which can be made on the same manufacturing scale as is electrolytic copper at the present time. It is of the highest degree of importance that a manufacturer should always be able to repeat the same quality of iron, and to secure uniformity in manufacture of transformers, when once the definite sizes and proportions of the circuits have been settled by laborious and, perhaps, expensive experiments; and it is very difficult indeed to do this as long as one of the important quantities concerned—namely, the permeability of the iron—remains a matter which may be subject to mysterious and inexplicable variations. Dr. Fleming.

Deferring for the present the consideration of Mr. Kapp's remarks until I deal with Mr. Crompton, I pass on to note that which fell from Mr. Evershed. I will point out to him that, although I join with him in admiration of Professor Ryan's work, yet that work left many points still unsettled. In particular, in Professor Ryan's experiments on the 10-light Westinghouse transformer, the iron losses are, by his figures, clearly shown not to be constant, but to diminish with increasing load, as will be seen by reference to the original paper; and the explanation of this was not given. The chief value of Professor Ryan's work was in showing us clearly the relative phases of the currents and electro-motive forces in the actual working transformer, which, up to that time, had been entirely a matter of guesswork.

Dr. Fleming. Moreover, we must not forget that precisely the same methods and experiments were published by Messrs. Duncan, Hutchinson, and Wilkes two years before, namely, in 1888. It is, however, satisfactory to have touched the bottom at last, and to have established, as a combined result of many workers' experiments, the fact that the iron losses in a transformer are, for all practical purposes, constant at all loads, and that the hysteresis losses are practically independent of the speed. Until these facts were established by reliable experimental methods, all conclusions made about transformers, and predeterminations of their performances, partook strongly of the character of clever guessing. Mr. Evershed concluded his remarks by expressing his regret that some of our Professors had not come in contact with a live transformer some years ago, and he concludes by rejoicing over one who has, he thinks, done so even at the eleventh hour. Perhaps it is only permissible to remind Mr. Evershed of the fact that the evolution of knowledge is to a large extent a matter of money. It often costs very little to propound a theory; it sometimes costs a good deal to arrive at a fact. It has become the fashion lately to cavil at the supposed tendency in scientific teachers to supply too much theory and too few facts. The manufacturers have the remedy largely in their own hands. Let them see that teachers who have the means and training to conduct experimental research are not starved in apparatus and the means of testing in practical work the theories they evolve. Some of the work of which Mr. Evershed has spoken in admiration was carried out by means of a complete alternator and transformer plant given by the Westinghouse Company to an American college. Can anyone point to an English college which has had a similar gift made to it by an English firm? Moreover, we do not find, as a general rule, that manufacturers are specially anxious to provide the Professors with results of experiments, by which alone they can test their theories. I am not saying that they ought to do so, because I am sufficiently in contact with practical work to know that manufacturers are not likely to give away the results of experiments, which may have cost them hundreds of pounds, to their commercial rivals; but, at any rate,

until they are prepared so to do, and to furnish their quota towards the progress of public information, it would probably be only bare justice for them to withhold destructive criticism on those who, at any rate, have furnished them freely with the best that they had at their disposal. Dr. Fleming

Mr. Mordey resumed the discussion on a subsequent evening, and he gave us the results of a number of exceedingly careful experiments upon the sudden rush into transformers which may occur at the moment of connecting them to the primary circuit. The results of all these experiments brought him to the conclusion that I was substantially correct, not only in the facts laid before you, but also in the general theories of the effect. There is nothing that calls for remark in these experiments other than to say how gratified I am that so competent an experimentalist as Mr. Mordey should have explored the effect thoroughly for himself and arrived at the confirmatory conclusions he has done. He has given us a number of additional facts to group with others in pointing the way to an explanation. In addition to this, Mr. Weekes and Mr. Haycraft have added other interesting observations made at the Central Institution, both on current-rushes into transformers and increased pressure on condensers. With regard to all this part of my paper, and the remarks made on it by speakers, I have only one observation to put forward. I wish to correct a view which I put forward in the paper that the rush into the transformer is an alternating rush. My present opinion is that the whole effect is over in less time than one period. Without dogmatising at all on what may require further elucidation, the following suggestions have occurred to me as a possible explanation of the whole of the observed effects :—

Consider a single magnetic cycle which is being traversed in the direction of the arrows by the iron (see Fig. 1).

Let us suppose that when the current finally ceases the magnetism left in the iron is represented by O A. When we close the circuit and start the current again, it must either begin to increase positively—that is, towards O C—or negatively—that is, towards O D. If the latter is the case, the magnetic cycle is picked up at the point where it was left, and continued in the

Dr. Fleming. proper direction, and the proper back electro-motive force due to rate of change of induction obtains at each instant. If, however,

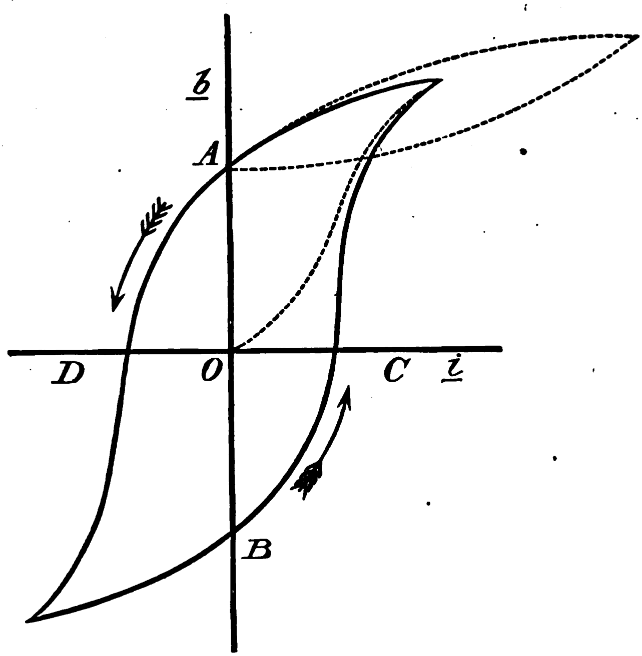


FIG. 1.

the current begins again to increase positively, then the true permanent magnetic cycle is not picked up at once, but a loop—the roughly probable form of which is shown by the dotted line—is formed on the magnetic cycle curve. This loop starts off at first nearly horizontally; in other words, the rate of change of induction with magnetising force, or $\frac{d\,b}{d\,i}$, is nearly or quite zero. Hence the back electro-motive force of self-induction, or $\frac{d\,b}{d\,t} = \frac{d\,b}{d\,i} \times \frac{d\,i}{d\,t}$, is nearly or quite zero, and, there being nothing to keep back the current, it comes in with a rush, until the reversal of electro-motive force restores things to their normal state.

It has been shown both theoretically and experimentally that sudden rushes of magnetising current, or force, do not produce induction in iron cores, or, at least, that such induction is at first

only skin deep, and soaks in slowly. Hence, if a current-rush begins, the total induction corresponding to any instantaneous current-strength is far below that value which would exist if that current-strength were maintained for a finite time; and the necessary result is that the time rate of change of induction is kept small. In general, therefore, there is no sufficient back electro-motive force to stop the rush, until the impressed electro-motive force changes sign and the magnetisation curve is again continued normally. Dr. Fleming.

If the circuit is broken slowly, so as to cause the magnetism of the iron to be entirely destroyed, then on beginning again we have to do, as it were, with virgin iron; and under these circumstances the value of the initial rate of change of induction with magnetising force, or $\frac{d\,b}{d\,i}$, is not zero for zero induction. Hence there can be no very great rush.

Dr. Sumpner followed, and he naturally put in a claim for his own method of testing transformers: no one can find fault with him for preferring the child of his own brain to that of any other. As a matter of fact, it is not generally, outside a factory, most convenient to test transformers in pairs. In most of the tests I have been called upon to make, only one transformer has been at disposal, and it has been necessary to do the best that I could with that. The method of testing a single transformer suggested by Dr. Sumpner would not give us all we wish to know in practice. For the purposes of a satisfactory test we require to know the iron loss, the copper losses in both circuits, and the currents and terminal voltages. We want to know, in the first place, the open-circuit losses of the transformer; and, in the second place, we want to know the copper losses at all loads,—so as to be able to calculate the all-day efficiency, as well as the secondary "drop." I have recently modified the dynamometer method described in the paper, and got rid of the large non-inductive resistance in the shunt coil, by making use of a transformer instead to supply the current for the shunt coil of the wattmeter. For this purpose I place a small transformer, T (see Fig. 2), across the primary mains, and connect the low-resistance shunt coil of the wattmeter

Dr. Fleming. through one or more incandescent lamps to the secondary terminals of this transformer. It is easily seen that, since the

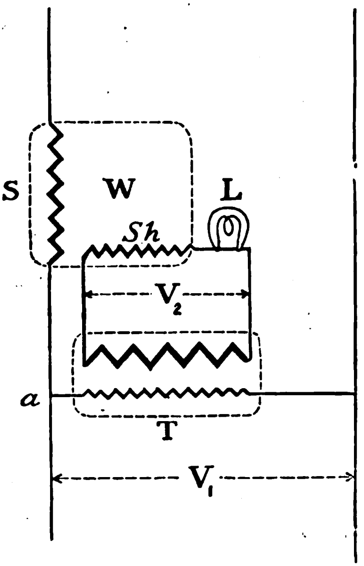


Fig. 2.

secondary current in a transformer is in exact opposition as regards phase with the terminal primary electro-motive force, this arrangement will give us a current through the shunt coil, *Sh*, of the wattmeter, W, which is in synchronism with the primary impressed electro-motive force. The arrangement, however, does not involve nearly so much power expenditure as when non-inductive shunt resistances are employed, as in the method described in the paper. Of course, where the cost of power is of importance, there the testing of transformers in pairs may be essential.

The really important point in Dr. Sumpner's speech was his investigation of the causes of error in the Swinburne wattmeter, and I think it is impossible to resist the logic of his experiments and conclusions. Fortunately, he possessed, in my paper, all the figures necessary for him to prove his view, and he has given interesting results, which seem quite sufficient to show that the whole of the defect in the first Swinburne wattmeter can be accounted for by local currents set up in the brass plate carrying the series coils, and that there is no need to assume any additional cause of error by reason of capacity. At any rate, his figures form the most conclusive proof of the importance of carefully avoiding the metal in the construction of wattmeters for alternating currents, especially metal in the close neighbourhood of the moving coil.

I now come to discuss Mr. Crompton's remarks, and I will take in conjunction with his those which fell from Mr. Wright as well.

Mr. Crompton did not restrict himself to the particular question before us—namely, the testing of transformers—but he followed out my paper into what he considered its logical consequences, from his point of view, and it led him to proclaim again the superiority of the low-pressure method of supply. There is no reason to find fault with him for this, because I inserted at the end of the paper a kind of invitation to the advocates of low-pressure systems to reconsider their case, and I felt sure Mr. Crompton would be likely to take this up. Mr. Crompton's speech contained matters which certainly have a very important bearing on questions with which this paper is concerned, because part of the object of that paper was to show the progress that had been made in the last seven years in the construction and design of transformers; and it seems to me, therefore, that some of Mr. Crompton's conclusions should not be allowed to pass without challenge. Dr. Fleming.

In some respects Mr. Wright is (if I may be pardoned for so saying) a more formidable opponent than Mr. Crompton, for, if anybody in this world understands the design of transformers, and what they will do, it is Mr. Arthur Wright; and since he has also had wide experience in the practical management of low-pressure stations, his endorsement of Mr. Crompton's contention that there is no room for an alternating-current station inside of a radius of a mile and a quarter, is one which, if allowed to pass unmet, would, in my opinion, leave the impression that the matter was beyond dispute. It may seem a very bold thing in me to join issue with two such authorities, but, nevertheless, I do say, and I think that it will be possible to prove to you by a demonstration that it is impossible to refute, that both of these gentlemen are incorrect in their conclusions, and that, so far from there being no room for an alternating-current station inside a radius of a mile and a quarter, it is simple matter of fact that a low-pressure feeder of a mile long, supplied on the usual load-factor, costs more to lay down and costs more to maintain than the high-pressure transformer feeder of the same capacity. This is the proposition that I propose to maintain before you, and in

Dr. Fleming. order to settle it we must make first some definite assumption as to the nature of the load. I shall assume that the load diagram of the supply is a 10 per cent. load diagram—that is to say, that it is a supply of the following nature:—

11 hours no supply;				
5 „	1-10th of full supply;			
4 „	1-8th	„	„	
3 „	1-4th	„	„	and
1 „	3-4ths	„	„	

If you work out this diagram, you will find that it is equal to $2\frac{1}{2}$ hours at full load, or to a load-factor of very nearly 10 per cent. No one can assert that this load-factor is an exceptional one. Having assumed this, I propose to demonstrate to you that if we take two feeders, each, say, of 24 kilowatts maximum capacity and a mile long, one a low-pressure three-wire feeder supplying at the business end at 200 volts across the outer mains, and the other a high-pressure feeder worked at 2,000 volts at the dynamo, and supplying a 24-kilowatt transformer at the business end to reduce to 100 volts, the high-pressure feeder will cost a less annual sum to maintain than the low-pressure feeder. In order to eliminate for the moment certain other essential matters, I ask you to assume that there are two stations side by side, both supplied with the same type of engine and boiler, and both supplied with the same coal, water, and oil, and managed by the same unbiassed engineer; but one station containing continuous-current dynamos, and the other alternators. The only demand I shall make is that we may take it that the alternators have the same electrical efficiency as the continuous-current dynamos. This has never been disproved, and Mr. Mordey's challenge on this point will be remembered. My object in this is to get rid, for the moment, of any differences as to mere cost of generation as far as engine-room expenses are concerned, and I shall take it that the cost of the unit generated at the dynamo terminals is the same in both cases. I say nothing at present about capital outlay for generating plant. This being so, be good enough to assume that, at a distance from this station, there is a town, divided into

two parts; in both parts low-pressure mains laid down, and unlimited lighting to be obtained. Let one half of the town be supplied by the three-wire feeders from low-pressure station, of the average length of one mile, and a capacity of 24 kilowatts as a maximum; and the other be supplied by high-pressure feeders of the same average length, each consisting of a high-pressure line of 7-16 in. section working at 2,000 volts, and a 24-kilowatt transformer in a pit at the town end supplying on to the low-pressure network. Dr Fleming.

I propose, then, to make comparisons between these two feeders, both as to energy wasted in the feeder in 24 hours, and as to the annual cost of maintaining the same.

Table IV.—All-Day Efficiency of Three-Wire Feeder one mile long, giving a maximum supply of 24 kilowatts at 200 volts constant pressure at the business end.

Let the diurnal load diagram be as above, viz.—

$$\left.\begin{array}{lll} 11 & \text{hours} & \text{no supply} \\ 5 & ,, & \text{1-10th full load} \\ 4 & ,, & \text{1-8th} \quad ,, \quad ,, \\ 3 & ,, & \text{1-4th} \quad ,, \quad ,, \\ 1 & ,, & \text{3-4ths} \quad ,, \quad ,, \end{array}\right\} = 10 \text{ per cent. load-factor.}$$

Case A.—40 per cent. drop at full load. Full current = 120 amperes in each main; resistance of each outer main = 0·33 ohm; cables, 37/16, 19/15, 37/16.

Feeder Loss in Watt-Hours.	Feeder Delivery in Watt-Hours.
$11 \times 0 \times \frac{2}{3} = 0$	
$5 \times (12)^2 \times \frac{2}{3} = 480$	$5 \times 12 \times 200 = 12{,}000$
$4 \times (15)^2 \times \frac{2}{3} = 600$	$4 \times 15 \times 200 = 12{,}000$
$3 \times (30)^2 \times \frac{2}{3} = 1{,}800$	$3 \times 30 \times 200 = 18{,}000$
$1 \times (90)^2 \times \frac{2}{3} = 5{,}400$	$1 \times 90 \times 200 = 18{,}000$
Total = 8·28 B.T.U.	Total = 60·000 B.T.U.

$$\text{All-day efficiency} = \frac{60}{68{\cdot}28} = 87 \text{ per cent.}$$

Case B.—30 per cent. drop at full load. Full current = 120

Dr. Fleming. amperes in each main; resistance of each outer main = 0·25 ohm; cables, 37/14, 19/14, 37/14.

Feeder Loss in Watt-Hours.	Feeder Delivery in Watt-Hours.
$11 \times 0 \times \frac{1}{2} = 0$	
$5 \times (12)^2 \times \frac{1}{2} = 360$	$5 \times 12 \times 200 = 12{,}000$
$4 \times (15)^2 \times \frac{1}{2} = 450$	$4 \times 15 \times 200 = 12{,}000$
$3 \times (30)^2 \times \frac{1}{2} = 1{,}350$	$3 \times 30 \times 200 = 18{,}000$
$1 \times (90)^2 \times \frac{1}{2} = 4{,}050$	$1 \times 90 \times 200 = 18{,}000$
Total = 6·310 B.T.U.	Total = 60·000 B.T.U.

$$\text{All-day efficiency} = \frac{60}{66 \cdot 31} = 90 \text{ per cent.}$$

Case C.—20 per cent. drop at full load. Resistance of each outer main = 0·165 ohm; cables, 37/13, 19/13, 37/13.

Feeder Loss in Watt-Hours.	Feeder Delivery in Watt-Hours.
$11 \times 0 \times \frac{1}{3} = 0$	
$5 \times (12)^2 \times \frac{1}{3} = 240$	$5 \times 12 \times 200 = 12{,}000$
$4 \times (15)^2 \times \frac{1}{3} = 300$	$4 \times 15 \times 200 = 12{,}000$
$3 \times (30)^2 \times \frac{1}{3} = 900$	$3 \times 30 \times 200 = 18{,}000$
$1 \times (90)^2 \times \frac{1}{3} = 2{,}700$	$1 \times 90 \times 200 = 18{,}000$
Total = 4·14 B.T.U.	Total = 60·000 B.T.U.

$$\text{All-day efficiency} = \frac{60}{64 \cdot 14} = 93 \text{ per cent.}$$

First consider the three-wire feeder. We must make certain assumptions as to the drop of pressure in that feeder. I have taken three cases, and in Table IV. have calculated out the diurnal losses in that feeder on this 10 per cent. diagram on the assumption of a 40 per cent. drop, a 30 per cent. drop, and a 20 per cent. drop respectively; the full-load current in each outer main being 120 amperes—the balance on the two sides being maintained—and the sizes of the mains required to give these drops being then calculated from the maximum current. Fix your attention, in the first place, upon Case A (40 per cent. drop—that is approximately what would be allowed in practice): you will see that the feeder loss in the 24 hours is 8·28 B.T.U., and the feeder delivery in the 24 hours is 60 B.T.U., or an all-day efficiency of 87 per cent.

Table V.—All-Day Efficiency of 24-Kilowatt Transformer on 10 per cent. Load Diagram, and Losses in Double 7/16 Primary Cable one mile long feeding the same. Dr. Fleming

Let the transformer have 1·35 per cent. loss on open secondary circuit, and 1·35 + 1·60 per cent. loss at full load. This gives an open-circuit loss of 324 watts, and an efficiency of 86 per cent. at 1-10th full load, and 96 per cent. at full load.

The energy balance-sheet for the transformer on the above load diagram is—

Energy in Watt Hours given to Primary Circuit.	Energy in Watt-Hours taken from Secondary Circuit.
$11 \times 324 = 3{,}564$	$11 \times 0 = 0$
$5 \times 2{,}762 = 13{,}810$	$5 \times 2{,}400 = 12{,}000$
$4 \times 3{,}372 = 13{,}488$	$4 \times 3{,}000 = 12{,}000$
$3 \times 6{,}420 = 19{,}260$	$3 \times 6{,}000 = 18{,}000$
$1 \times 18{,}612 = 18{,}612$	$1 \times 18{,}000 = 18{,}000$
Total = 68·734 B.T.U.	Total = 60·000 B.T.U.

The primary cable losses ($C^2 R$) on same load in watt-hours are—

$$
\begin{aligned}
11 \times 4 \times (\tfrac{1}{8})^2 &= 1 \\
5 \times 4 \times (1)^2 &= 20 \\
4 \times 4 \times (1\tfrac{1}{4})^2 &= 25 \\
3 \times 4 \times (2\tfrac{1}{2})^2 &= 75 \\
1 \times 4 \times (7\tfrac{1}{2})^2 &= 225 \\
\text{Total} &= 0{\cdot}346 \text{ B.T.U.}
\end{aligned}
$$

Total loss in 24 hours in cable and transformer = 8·734 + 0·346 = 9·08 B.T.U.

All-day efficiency = 87 per cent.

In Table V. I have calculated the diurnal losses in the high-tension feeder consisting of a mile of double 7/16 cable and a 24-kilowatt transformer, and you will see that, on the assumptions made as to that transformer, the diurnal loss is 9 B.T.U. in the cable and transformer, and the delivery 60 B.T.U., or an all-day efficiency of 87 per cent.

Dr. Fleming. *Table VI.—All-Day Efficiency of Pair of 12-Kilowatt Transformers on a 10 per cent. Load Diagram, and also Energy Losses in a Double 7/16 Primary Cable one mile long feeding the same.*

The energy balance-sheet of the transformers (Ferranti), as taken from tables of observation, is as follows:—

Energy in Watt-Hours given to Primary Circuit.	Energy in Watt-Hours taken from Secondary Circuit.
$11 \times 2 \times 149 = 3{,}278$	$11 \times 2 \times 0 = 0$
$5 \times 2 \times 1{,}189 = 11{,}890$	$5 \times 2 \times 1{,}055 = 10{,}350$
$4 \times 2 \times 1{,}580 = 12{,}640$	$4 \times 2 \times 1{,}422 = 11{,}376$
$3 \times 2 \times 3{,}064 = 18{,}384$	$3 \times 2 \times 2{,}903 = 17{,}418$
$1 \times 2 \times 8{,}932 = 17{,}864$	$1 \times 2 \times 8{,}614 = 17{,}228$
Total = 64·056 B.T.U.	Total = 56·372 B.T.U.

The primary cable losses ($C^2 R$) on the same load in watt-hours are—

$$11 \times (0{\cdot}076 \times 2)^2 \times 4 = 1$$
$$5 \times (0{\cdot}503 \times 2)^2 \times 4 = 20$$
$$4 \times (0{\cdot}675 \times 2)^2 \times 4 = 29$$
$$3 \times (1{\cdot}265 \times 2)^2 \times 4 = 77$$
$$1 \times (3{\cdot}713 \times 2)^2 \times 4 = 237$$

Total = 0·364 B.T.U.

Total loss in 24 hours in cable and transformers = 7·684 + 0·364 = 8·049 B.T.U.

All-day efficiency = 88 per cent.

In Table VI. I have calculated in the same way the high-tension feeder efficiency on the assumption that the single transformer is replaced by two 12-kilowatt transformers, and you will see from that table that the total loss in the transformer and cable on the 10 per cent. load diagram is 8·049 B.T.U., or an all-day efficiency of 88 per cent. Even better results than this can now be obtained. Since reading my paper I have had tests taken of a 25-H.P. transformer, which showed 90 per cent. efficiency at 1-10th of full load. I have also received from Mr.

Mordey the figures of observation made on a 24-kilowatt transformer by the dynamometer method, and which are as follows:— Dr. Fleming
Open circuit loss = 240 watts; loss in copper at full load = 480 watts. These figures are equivalent to an efficiency at full load of 98 per cent., and an efficiency at 1-10th of full load of 89·8 per cent. On a 10 per cent. load-factor as above described, these figures show that this transformer would have 90 per cent. all-day efficiency. Accordingly, at the distance of one mile on this load diagram, a transformer feeder of the kind supposed is equivalent, as far as regards energy losses, to the low-pressure feeder, and both of them may be taken to waste 8 units per day in delivering 60 units on the 10 per cent. load diagram. The next thing that remains to be done is to estimate the relative capital outlay on these two feeders, and to fix figures for the depreciation and interest on the same. In doing this I have not relied on my own estimates, but I have gone to responsible people and asked them to furnish me with figures at which they would be prepared to carry out the work of laying these feeders respectively. The specification for the high-pressure feeders is as follows:—

The primary cable to be a concentric cable, each conductor having a section equal to 7/16, the resistance of the go and return—namely, two miles—being 4 ohms. Vulcanised india-rubber to be used as insulation, taped and braided; cable to be drawn into a wrought-iron pipe, provided with 20 draw-boxes; a pit at the business end for the transformer, which is to be a 24-kilowatt transformer with a loss on open circuit of 1·35 per cent., and a loss on full load of 3 per cent.—which, you will find, gives an efficiency of 86 per cent. at 1-10th full load, and 96 at full load.

The estimate for that work complete, laid in the ground, but exclusive of the cost of opening and making good the road, is £465.

For the low-pressure feeder the specification was as follows:—

Low-pressure feeder to consist of three cables, each one mile in length, respectively 37/16, 19/15, and 37/16, giving a drop of 40 per cent. when 120 amperes are flowing in cable and when 200 volts are maintained at the business end between the

Dr. Fleming. outer cables. The cables to consist of Callender's bitumen lead-covered and steel-armoured cable, laid in the ground direct. Total cost of laying one mile of three-wire feeder, exclusive of cost of opening and making good the road, £925.

I have checked these figures by comparing them with a number of other makers of cables and transformers. Without giving you the actual detailed figures, which I am perhaps not warranted in doing in these cases, I may say that I have considered the cost of laying the high-pressure feeder with Siemens concentric cable, with Fowler-Waring cable drawn into iron pipes, and with Callender cable laid on the solid system of iron troughing and bitumen, and all these figures lie between £400 and £470 for the mile of high-pressure line, exclusive of the cost of opening the ground. The transformers by different makers may vary in price from £3 to £4 per kilowatt.

With regard to the low-pressure feeder, I have considered the cost of laying it both with Siemens concentric cable and with Callender cable drawn in Callender-Webber casing, laid in the ground; and the estimate for this work, exclusive of the cost of opening and making good the ground in all cases, lies between £900 and £1,000. I shall, therefore, take it as a perfectly certain fact that the cost of that low-pressure feeder, laid, apart from the opening and making good of the ground, would cost £1,000 per mile; that the transformer feeder would cost not far from £500—£400 for cable, and £100 for transformer and pit.

We have, then, to compare the annual expenditure on these two feeders respectively, assuming that the above 10 per cent. diagram represents the everyday diagram throughout the year. Since the loss in both cases is the same—namely, 8 units per diem—if we take the manufacturing cost of this at 3d. per unit, it will give us a total annual cost for power wasted in the two cases of £36 10s. To this must be added 5 per cent. on the capital outlay for interest, which, taking the capital outlay on the low-pressure feeder as £1,000, and the capital outlay on the transformer feeder as £500, amounts respectively to £50 and £25. This is exclusive of the annual interest on the cost of excavation and making good the road, which may perhaps be taken as about

the same in the two cases—the amounts cancelling in comparing the two annual expenditures. Dr. Fleming

The next question which faces us is to agree upon the depreciation to be placed respectively against the several items. Here we shall find great differences of opinion; but even if I take figures which may be considered unfair to the transformer feeder, nevertheless, having a strong case, I can afford to be generous. I shall take the depreciation on the transformer as 15 per cent., at which I know many makers of transformers will exclaim, as putting the transformer on an equality with a secondary battery. Placing, then, the cost of the transformer at £100, this amounts to £15. On the high-pressure cable I shall take a depreciation of 5 per cent. on £400, making £20 per annum. On the low-pressure cable I shall take the depreciation at the exceedingly moderate amount of 2½ per cent., and I have no doubt that at this figure manufacturers of high-pressure cables will consider that I am doing them an injustice; but I wish to put my case first in a manner which will render it impossible for the supporters of low tension to refute it.

We have, then, the following comparative annual costs:—

	Low-Pressure Feeder. £	s.	High-Pressure Feeder. £	s.
Annual cost of energy wasted in both cases at 3d. per unit ...	36	10	36	10
Interest on capital outlay, at 5 per cent.	50	0	25	0
Depreciation—				
2½ per cent. on the low-pressure feeder	25	0	—	
5 per cent. on the H.P. feeder ...	—		20	0
15 per cent. on transformer ...	—		15	0
	£111	10	£96	10

I have no doubt that the majority of those who are able to form an opinion would say that the depreciation of the high-pressure cable ought not to be taken at as high a percentage as that of the low-tension cable, far less at double the value; but you may adjust these numbers as you please: as long as you keep within

Dr. Fleming. the limits of practical experience, you will find this to be demonstrated without possibility of refutation—that the annual cost of this high-pressure feeder of one mile length, delivering 24 kilowatts as a maximum load, is less considerably than the annual cost of maintenance of the low-pressure feeder, and, in particular, that the capital outlay for the low-pressure feeder is about double that of the high-pressure feeder. If you take any of the other tables, and calculate, for instance, what would be the annual cost of the low-pressure feeder on the assumption that you take a 20 per cent. drop instead of a 40 per cent. drop, you will find that, while the annual cost of the energy wasted is reduced to about £18 per annum, the capital outlay in the low-pressure feeder, exclusive of excavation and making good, will be increased by 50 or 60 per cent.

It will be found that the tender for the low-pressure feeders of the necessary section to give 20 per cent. drop would vary between £1,550 and £1,800, according to the system employed, and, therefore, that the total annual sum to be provided for depreciation and interest, on the assumption that that feeder with 20 per cent. drop, which would consist of three cables—37/13, 19/13, and 37/13 respectively—would amount to £85 and £42, or to £127 instead of £75; so the result would be to save £18 per annum in wasted energy, and to add £52 per annum for depreciation and interest—in other words, to increase the total annual cost. These figures, which I have endeavoured in every way to subject to test, do not in the least degree confirm the statements of Mr. Wright and of Mr. Crompton that there is no room inside a radius of a mile and a quarter for an alternating-current station; on the other hand, they show most decidedly that there is no room outside a radius of one mile for a continuous-current station; and the more carefully you look into the matter, the more will you find that the evidence which is at our disposal for deciding this question leads you to the firm conclusion that the economical radius of supply by three-wire direct-current systems is reached at distances when the feeders have an average length of somewhat less than one mile, with load diagrams of 10 per cent. But the case becomes much more unfavourable to the

low-pressure supply if we suppose the load diagram to improve. We do not build our stations with the object or hope of remaining permanently with load-factors of 10 per cent. No one is making greater efforts than Mr. Crompton to increase these abnormally low load-factors. Examine, then, what will be the result of improving the load-factor: in every case it must be to diminish the efficiency of the low-pressure feeder. This is clearly obvious, because, if the load-factor rises to 100 per cent., the efficiency of the feeder with the 40 per cent. drop would only become something like 70 per cent.; and this is the limit towards which the efficiency of distribution of the low-pressure system tends, as the load-factors are progressively increased when secondary batteries are not used. Who shall say what it falls to when secondary batteries (whatever their additional advantages) are employed? On the other hand, it is a matter of simple demonstration that in proportion as the load-factors increase, the efficiency of distribution of the high-pressure feeder must tend as a limit towards 95 per cent.—2 per cent. being the drop in the line, and 3 per cent. the loss in the transformer. In all cases, therefore, in which large load-factors are expected to be found, we see the wisdom of the selection of an alternating-current system. Those of you who know what the average load diagram of one of the City of London stations is like, will see how well-advised have been the authorities who settled and selected an alternating-current system for the lighting of the City of London. Dr. Fleming

In all the above discussion, I have altogether left out of account for the present moment the capital outlay on the generating plant, but there is an important point which was suggested to me by Mr. Mordey in conversation, which must not be lost sight of in this matter. To supply a maximum of 100 kilowatts at the business end of a high-pressure feeder, we have only to put down a generating plant capable of supplying, at the maximum, 105 kilowatts at the dynamo end, because the losses at full load are only 5 per cent.; but if we select a low-pressure feeder to do the same work, we have to put down generating plant with a capacity of 140 kilowatts as its maximum output, because the maximum waste of 40 per cent. occurs in the low-

Dr. Fleming. pressure feeder at the very hour of maximum demand at the business end. This, of course, very seriously affects not only the relative whole capital outlay in generating plant, but the relative whole annual costs of the system, and opens up a view of the case which must not be lost sight of in considering all these questions. In fact, the enthusiastic advocates of low-pressure working have done their utmost to draw the attention of the world to the losses occurring in transformers, but they have judiciously kept exceedingly silent on the equally important matter of the losses in low-pressure feeders.

I find, on examining the printed report of Professor Kennedy's interesting remarks, that he has worked out, to some very great extent, the all-day losses in transformers on various load diagrams which are derived from exceedingly wide practical experience of the needs of different classes of customers. It is impossible to deny that the system of house transformers is one in which the all-day losses are exceedingly large, but none of us who at any time have advocated alternating-current systems have ever looked upon that house-to-house system as anything more than necessary in initial stages of supply. In this case, where the alternating-current stations have been or are being laid out with knowledge and skill and experience from the beginning, nothing but a system of transformer feeders and low-pressure secondary networks is being adopted; and when we come to collate the results of sufficient experience of the working of that system with the results of working of low-pressure systems, there is no reason to believe that the balance of advantage would be found on the side of the continuous-current system. Up to the present, the figures that have been available for comparison have been entirely drawn from acknowledged imperfect systems of alternating-current supply, and hence we have never been able to institute any fair and proper comparison between the two systems. The figures, however, that I have given you above, and others which you will be able for yourselves to draw from the facts in my paper, will be amply sufficient to show you that there is no ground whatever for that view of the future of alternating-current working which Mr. Crompton pressed on us, not only in the course of this discussion, but on several other occasions.

There is one other point to which I ought to draw your attention, and that is, that even where we are dealing with the same percentage load-factor there may be a great difference between the efficiency of distribution by a feeder when the diagram form alters, even if it still includes the same area. To make my meaning clear, consider a load diagram of the following kind:— Dr Fleming.

9 hours at no load;
8 „ „ 1-8th of full load;
3 „ „ 1-4th „ „
2 „ „ 1-2 „ „
2 „ „ 3-4ths „ „

This is equivalent in area to $4\frac{1}{4}$ hours at full load, or to an 18 per cent. load-factor. If you take the low-pressure three-wire feeder in the case A above, and calculate the feeder loss and feeder delivery in 24 hours, you will find them to be 18·6 units and 102 units respectively on this diagram, or a feeder efficiency of 84·5 per cent. If, however, you take the same percentage load-factor—viz., 18 per cent.—and consider it as made up by $4\frac{1}{4}$ hours at full load and $19\frac{3}{4}$ hours at no load, then, on calculating the feeder loss and feeder delivery, they will be found to be 40·8 units and 102 units. Hence, with the same percentage load-factor, the efficiency of distribution has now fallen to 70 per cent. On the contrary, a transformer feeder would have benefited in efficiency by the concentration of the load. One of the most remarkable qualities of a low-pressure feeder is the degree to which a very moderate duration of full load depresses the efficiency of distribution. The above 18 per cent. load-factor, when taken as $4\frac{1}{4}$ hours at full load, is quite as effective in lowering the efficiency of the feeder as a 24-hour supply at full load would be. In this last case the same feeder supply would be 576 units, and the feeder loss 230 units, or the efficiency 70 per cent. If, however, we consider the alternating-current feeder, the above facts are conversely illustrated. The higher or more concentrated the load-factor, the better the efficiency of distribution. I need not go on illustrating or expanding these facts. I take it to be demonstrated that on such a 10 per cent. load-factor as I have taken, the economical radius

Dr. Fleming of transformation is reached at less than one mile. That is to say, if electric energy has to be delivered on such a load diagram at an average distance of one mile, it can be done more cheaply, both in first cost and annual outlay, by a transformer feeder than by a low-pressure three-wire feeder.

It is clear also that, in spite of contrary views, this economical radius of transformation will be decreased as the load-factor increases; and that, unless all the circumstances of the load are stated, we are not in a position to definitely say which method has the advantage in point of economy at distances less than one mile. My object in the paper discussed has been to place such facts before you as will enable you to pursue this inquiry yourselves, and to rid the subject, at least in part, of the ambiguities and doubts in which it has been enveloped. Lord Bacon tells us that "truth emerges more easily from error than from confu-"sion;" and if the outcome of the work, and the discussion which has taken place on it, is in any degree helpful in furthering our knowledge of the subject of transformer working, and dispelling illusions or confusions with which it may be surrounded, it will not prove a disadvantage for you to have so fully discussed the paper you have done me the honour to receive.

The President. The President: Gentlemen,—I am sure I shall only be expressing the opinion of all of you in saying that we are deeply indebted to Mr. Fleming for having brought this subject before us in a paper which I believe will be classical, and for having made it a great deal more interesting and amusing by his spirited reply to-night.

For myself, I pretend to be, and I believe I am, an absolutely unprejudiced individual. I have no interest, directly or indirectly, in any alternating-current system or direct-current system. My experience is entirely in the direct continuous system: I have under my immediate charge two very large stations that are worked on the direct continuous-current system; but after a great deal of study and a great deal of investigation, with a free hand and an unprejudiced mind, I have come to exactly the same conclusion as that which Dr. Fleming has given us to-night—viz., that while the continuous current can have its day within a mile

it stands no chance against the alternating current outside that mile. Gentlemen, we are deeply thankful, as I said, to Dr. Fleming, and shall read with interest his paper and remarks in print. The President.

The scrutineers report the following candidates to have been duly elected:—

Members:

Robt. Henry Smith (Professor).	Gerald Stoney, B.A.

Everden James Wimshurst.

Associates:

Hans Hamilton Benn, B.A.	Noel Francis Nalder.
Arthur B. Chatwood	James Pigg.
William Havilland Druce.	Arthur E. Pond.
William Martin Evans.	James Roberts, M.A., LL.B.
Henry Farmer.	Keith Robinson.
Frederic James Madgen.	Leigh Robinson.

Students:

William Cotsworth.	Thomas Richard Kenny.
William Richard Thomas Cottrell.	John Peacock Mackenzie.
John R. Dick.	James Henry Millen.
Richard Thomas Durran.	Wilfred John Previté Orton.
Allan Bertram Field.	George Herbert Oswald.
Edward Graham Fleming.	Hastings Victor Sadler.
Andrew Gray.	Phillip J. Sageman.
E. E. Gunter.	Charles Davis Taite.
Joseph Renwick Hewitson.	William Henry Wilding.
	William Heselton Wraith.

The meeting then adjourned.

The Two Hundred and Forty-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 23rd, 1893—Mr. W. H. PREECE, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting, held on the 9th February, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members.

Robert Hammond.	George Hinde Nisbett.

From the class of Students to that of Associates.

Hermann Clark Haycraft.	Frederick George Shrewsbury.

Louis John Steele.

Mr. Fairfax and Mr. Haslam were appointed scrutineers of the ballot.

The PRESIDENT: I now call on Mr. Mordey to read his paper.

ON TESTING AND WORKING ALTERNATORS.

By W. M. MORDEY, Member.

Mr. Mordey. In connection with the testing of large dynamos, the provision of the necessary power is often a very serious matter; and the driving plant for this purpose is a heavy item in the equipment of a dynamo factory, especially as, in order to get the full output from machines of sizes that are now often made,

much larger boilers and engines are required than for the works where the machines are made. Mr. Mordey

Very little has been written about the actual testing of alternators, and, so far as I am aware, they have always been tested in the manner commonly followed with direct-current dynamos—that is to say, they have been run on circuits of lamps, or on wire or other resistances, either directly or through transformers. Where it is desired to test the prime motor as well as the alternator there is no need to do more than this; but when engine and boiler powerful enough are not available, or when it is desired to carry out long runs at the least cost in plant and fuel, other methods have to be sought for, and we naturally turn for assistance in this matter to the method of circulating power first devised and described by Dr. Hopkinson.

It may be of interest to first glance briefly over the subject of testing by this method.

In Dr. Hopkinson's tests, published in 1886,* two similar dynamos were mechanically and electrically coupled. One ran as a generator supplying current to the other, which then acted as a motor and helped to drive the generator. It was necessary that the coupled machines should be driven by some source of power sufficient to provide for their internal losses. It was further necessary, in order that the circulation should take place, that one machine should have a higher E.M.F. than the other. When similar machines were used, this was readily arranged by weakening the field of the motor.

Dr. Hopkinson introduced this arrangement with the special object of attaining a high degree of accuracy in determining the efficiency of dynamos. It is comparatively easy to obtain accuracy in measuring the electrical output or input of generator or motor. To measure the input or output of power mechanically is more difficult. Dr. Hopkinson reduced this difficulty by reducing the power required to be measured mechanically to less than 1-10th of what it would be under ordinary circumstances.

Lord Rayleigh† proposed to supply power to the coupled dynamos

* *Phil. Trans.*, 1886, vol. ii., p. 347.

† *El. Review*, vol. xviii., p. 242.

Mr. Mordey by an accumulator or small dynamo instead of by a belt, thus making the method completely electrical.

This method, in one form or another, has been frequently used for testing the efficiency of direct-current dynamos. When two similar dynamos are available it is very useful, although the positive coupling is rather a drawback. If a belt is used, then the loss in the belt is included in the power required to be furnished to the combination. By running with and without belt or other gearing we have a simple way of finding belt or gearing losses—a matter that is involved in some obscurity. For this, as for other purposes, the method has the advantage that the losses in the mechanical transmission of large powers may be tested without the absorption or generation of large power.

But quite apart from the testing of efficiency this method has a good quality of a very solid kind, as it enables large dynamos to be run at full load by small engines. To the manufacturer this is often of great importance. It is necessary or advisable to run machines at or over their full load for a sufficient time to ascertain their mechanical qualities and how much they heat, and so on. And on the principle of Dr. Hopkinson's method small engines may be used, as it is only necessary to circulate the power by placing motors on the dynamo circuit instead of lamps or resistances, and causing these motors to drive back on the engine, countershaft, or dynamo. The engine then has to supply only power sufficient to cover the conversion and transmission losses. I do not think advantage is taken of this as often as it should be.

In the case of dynamotors or of high- and low-pressure machines, the method may be applied by coupling the armatures together, high pressure to high pressure, and low pressure to low pressure, and supplying only power to cover the losses.

In some cases large direct-current dynamos have been made with two armature windings and two commutators. It will be seen that a complete test may be taken with a single machine of this kind by circulating the power within the machine, adding an E.M.F. sufficient to enable the generator half to force the full current against the E.M.F. of the motor half. The power

so added gives the internal losses, and the efficiency is of course readily ascertained. A single dynamotor cannot be tested in this way, as the two windings are usually very dissimilar. Mr. Mordey

So far as I know, the first application of Dr. Hopkinson's method to alternate-current work was by Dr. Sumpner,* who, some months ago, worked out and published a very interesting plan for testing transformers in pairs by circulating the power. When it is remembered that the full-load efficiency of transformers is something like 96 or 97 per cent., it will not be necessary that I should dwell on the economy of this plan.

Dr. Hopkinson's method may be applied to two alternators coupled mechanically and driven as in his original test of direct-current dynamos. It is, however, much more convenient to be able to run machines singly. The necessary conditions for this are readily realised in an alternator, especially if the armature is stationary; and, in this way, I wish to show, in the first place, that it is possible to run an alternator under all the essential conditions of full load with only a small absorption of power. This may be done simply by dividing the armature circuit into two portions and so connecting them together that one has a higher E.M.F. than the other, the stronger portion acting as a generator, absorbing power, and driving the current through the weaker portion, which acts as a motor, returning the power to the generator portion, less the losses.

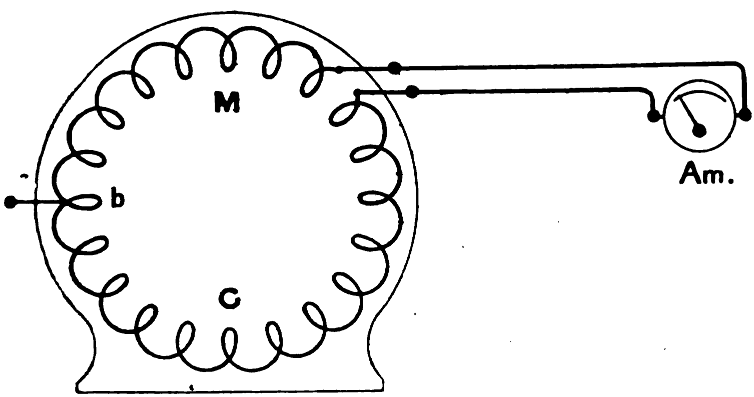


FIG. 1.

* *Electrician*, vol. xxix., p. 223, July 1, 1892.

Mr. Mordey. The two portions of the armature may be made of unequal E.M.F. by dividing the armature into two unequal portions, as shown by Fig. 1, which represents the armature of an alternator. The coils are joined up so that the portion M is opposed to the portion G. By making G greater than M, and by running with the field magnet excited, any required current may be made to circulate round the whole armature. This current is measured by an ammeter put in at any part of the circuit. The volts of each coil, or the volts across the junction of the portions M and G, may be taken in the usual way. When the full current is circulating, with the full field and full speed, the machine will be working at full load, so far as mechanical strains and electrical losses are concerned, and the power to drive it will be merely that required to make up for the mechanical and electrical losses. The efficiency is then simply found.

Test of an "A 20" 250-Kilowatt Alternator.

The machine was driven by an engine from which indicator diagrams were obtained. It was found that the I.K.W. was 3 kilowatts more when excited than when not excited. This is lost in eddies. The armature was then joined up in two portions, as in Fig. 1, and a current of 60·25 amperes circulated, with a P.D. of 1,656 volts across the junction of the two. This was equivalent, so far as internal losses were concerned, to an output of $1{,}656 \times 60{\cdot}25 \times 2 = 199{\cdot}5$ kilowatts. The power absorbed was 6·37 kilowatts more than when not excited. As the power wasted in eddies was 3 kilowatts, the loss due to the circulation of the current is $6{\cdot}37 - 3 = 3{\cdot}37$ kilowatts. To check this, we may examine the resistance. The armature has a resistance of 0·23 ω when the two sides are parallel, but as all the coils were in series in this test, the resistance was $0{\cdot}23 \times 4 = 0{\cdot}92\ \omega$. We will call this 1 ω to allow for a slight increase due to heating. The $C^2 R$ loss thus would be $60{\cdot}25^2 = 3{\cdot}63$ kilowatts. This is a very fair confirmation. As a matter of opinion, I have very little doubt that the eddies are less under load than on open circuit.

To ascertain the efficiency we have to add the excitation and the friction. The excitation was 1·365 kilowatts. The

friction is the only quantity not determined, as it could only be obtained as part of the total friction, including engines, ropes, and alternator. But I have put down 3 kilowatts for alternator friction, and this will no doubt be considered fair, as there was scarcely any perceptible warming of the bearings. A good deal of air is moved by the magnet, but we know that in this very little power is spent. The losses are: Mr. Mordey

	H.P.	K.W.
Friction	4	3
Excitation	1·83	1·365
C R and eddies ...	8·54	6·37
Total ...	14·37	10·735
Output...	267·47	199·5

Commercial efficiency—

$$\frac{199·5}{199·5 + 10·735} = 0·9489.$$

The "electrical efficiency"

$$= \frac{3·63 + 1·365}{199·5 + 3·63 + 1·365} = 0·9675.$$

In the above I have expressed power in kilowatts instead of horse-power, not merely out of deference to the President, but because almost all the quantities are actually determined and required to be known in kilowatts, and because I think it the most simple and convenient way.

Another way to upset the balance between the two portions of the armature is shown by Fig. 2.

The secondary of a transformer is interposed in the armature circuit, the primary of this transformer being connected across the junction of the two portions, or wherever convenient. The two portions of the armature have equal and opposite E.M.F.'s, and the transformer secondary provides the E.M.F. necessary to cause the circulation. It is only necessary, as in the first case, to run the machine with a full field to produce in it all the conditions of full load.

I have only tried this plan to see that it works, but have not used it for any actual tests.

Mr. Mordey. The unbalancing E.M.F. may of course be supplied by another alternator instead of by the machine under test.

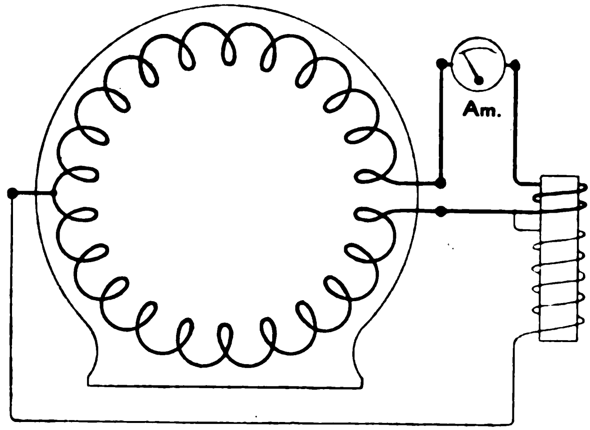


Fig. 2.

In these methods of running and of testing, the power required to drive the alternator has to be supplied and measured mechanically. This may be done by a transmission dynamometer, or by indicator diagrams of the engine. Neither of these methods is quite satisfactory, or quite direct enough. Transmission dynamometers are not always at hand, and, when available, a good deal of careful preliminary work is needed to get accurate results. Indicator diagrams are not very satisfactory. The power which is required to be known may be only a small part of that actually developed, and, especially with large engines running on light load, diagrams are not easily read with any high degree of accuracy. A better plan would be to use an electromotor to drive the alternator, especially if directly coupled so as to avoid the use of belts. The power supplied to the motor may be measured electrically, and the necessary deduction made for the motor.

A direct electrical measurement of the whole of the losses is, however, the only one that can be considered fully satisfactory. One way in which this may be obtained is shown by Fig. 3.

This arrangement reduces the power necessary to test a machine at full load to rather more than half the usual amount.

An alternator armature, A′, is divided into two portions, M and G. The machine is run up to synchronism with another Mr. Mordey.

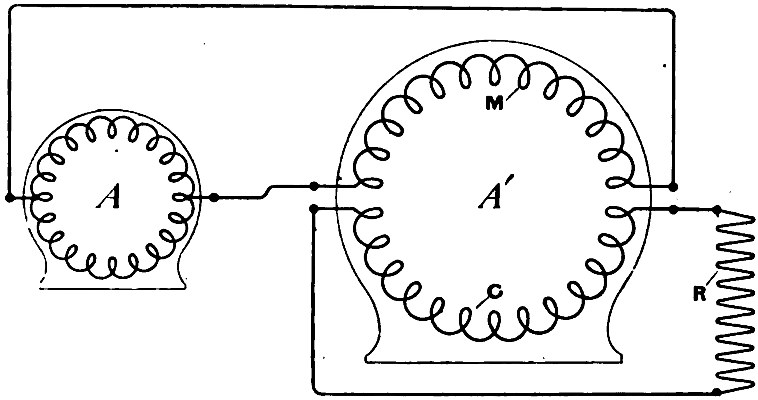


FIG. 3.

alternator, A, which may be of about half the capacity of the machine to be tested. The part M of the machine A′ is then run as a motor, while the part G is used as a generator doing work on an external circuit, R. The belt is thrown off. The machine acts, in short, exactly in the manner of an ordinary direct-current dynamotor. The input and output are measured by wattmeters or otherwise across the respective terminals.

The total losses = input + excitation − output.

$$\text{The efficiency as a dynamotor} = \frac{\text{output}}{\text{input} + \text{excitation}}.$$

When the generator portion is working at half the normal output of the alternator, the motor half will be working at a little more than its full load, and the internal losses will be the same as if the machine were working at full output (the only difference being that the friction losses are put in electrically instead of mechanically). The full-load efficiency under these

$$\text{circumstances} = \frac{\text{output} \times 2}{\text{input} + \text{output} + \text{excitation}}.$$

It will be noticed that this involves the assumption that the efficiency of the machine as a motor is the same as when used as a generator—an assumption that is fair.

Mr. Mordey. Some particulars of a test of an "A 10" 50-kilowatt alternator arranged in the manner shown in Fig. 3 are given below.

The machine was separately excited and driven up to synchronism with a 25-kilowatt alternator. One half, M, of the armature was then run as a motor, and readings were taken of the current, volts, and watts—the latter by a modified Siemens electro-dynamometer, used as described by Dr. Fleming, and having an added resistance of 1,000 ohms of non-inductive platinoid wire capable of easily carrying a current of 2 amperes.

The generator portion, G, was run on a circuit of straight platinoid wire, and the output was measured in amperes and volts. The circuit was one that had been in use for a long time, and had been carefully tested and found to be non-inductive and without sensible capacity. The test of this circuit, as of others in which a similar quality was desired, consisted simply in ascertaining that a given P.D., whether A.C. or D.C., gave similar current readings on a Siemens dynamometer.

True watts are therefore correctly given by $C' \times V'$.

The field was separately excited, and the current and volts measured. Speed was constant at 600 revs., = 100 ∿.

TEST OF "A 10" ALTERNATOR.

Motor Portion.					Generator Portion.			Excitation.				
C.	V.	Watt-meter, W.	C × V.	Power-Factor $\frac{W}{C \times V}$	C'.	V'.	C' × V'.	Watts, *w*.	W + *w*.	Dynamotor Efficiency, $\frac{C' \times V'}{W + w}$	Losses, W + *w* − (C' × V')	Commercial Efficiency, $\frac{2C' \times V'}{W + (C' \times V') + w}$
8·023	2,000	15,344	16,046	0·95	6·9	1,960	13,584	420	15,764	0·868	2,180	0·9257
9·278	2,000	17,628	18,556	0·95	8·1	1,950	15,855	427	18,055	0·878	2,200	0·935
10·5	2,000	19,950	21,000	0·95	9·3	1,940	18,102	427	20,380	0·89	2,278	0·941
10·22	2,000	19,418	20,440	0·95	8·6	2,040	17,604	465·9	19,884	0·885	2,280	0·938

Another way, arising naturally out of Fig. 3, is to circulate the power given by the portion G back to the circuit of A to M instead of wasting it on the resistance, R. This may be done by putting one winding of a transformer of suitable size in place of the resistance, R, and a second winding of the transformer in the

circuit between A and M. In order that this should succeed, it is necessary that the transformer should be capable of allowing a sufficient current to pass through one winding alone to run the machine as a motor lightly loaded. Then the portion M will be traversed by a current larger than that produced by G, otherwise the machine would not run as a motor. Mr. Mordey.

Another way, which has the advantage of being a complete electrical method, is shown by Fig. 4.

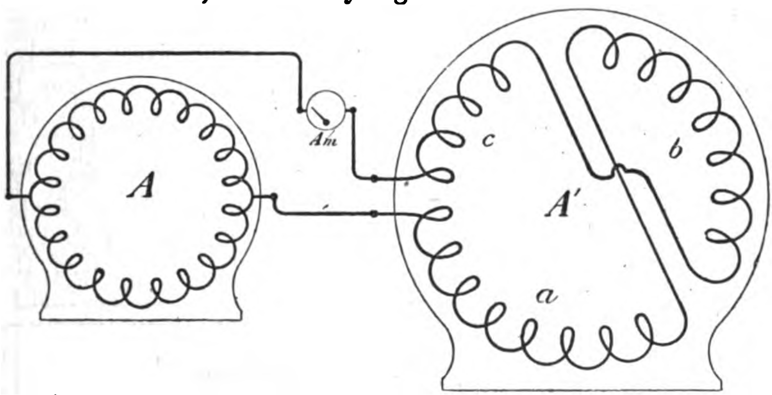


Fig. 4.

The armature, A′, of the alternator to be tested is divided into, say, three portions—*a*, *b*, and *c*—which are so joined up that the portions *a* and *b* neutralise one another so far as E.M.F. and power are concerned. Then the machine is run up to synchronism with the generator A, which is made to supply current to A′, and the belt is thrown off. It will be evident that A′ runs as a motor purely by virtue of the power of the portion *c*. The portions *a* and *b* do nothing. If the field of A′ is suitably excited and the portion *c* is correctly proportioned, the full working current may be forced through the whole armature, and all the internal conditions of full load will be observed, with the exception that—as in the other dynamotor methods—the torque will not be in the same sense all round the armature—a matter that does not interfere with the accuracy and value of the test. A single watt-meter reading taken at the terminals will then give the whole of the power expended in the machine, including mechanical and electrical friction and eddies, as well as excitation if the exciter is driven by the machine.

Mr. Mordey. Other ways of driving the alternator electrically, and of testing it under the conditions of full load, will readily suggest themselves.

For instance, a portion of the armature may be separated from the remainder and supplied with power from an external synchronous source sufficient to run the whole machine, and the portions not so used may be used, as in Fig. 1 or Fig. 2, to circulate power within the machine. In such an arrangement, however, the different portions of the armature are not equally loaded. Running at full current and small power may also be done by using simple choking coils for the external circuit, but this introduces disturbing influences which make it of very little use.

Another way of testing alternators economically may be mentioned. Two years ago* I described an experiment made with one of my alternators, showing that if one coil of the armature is left out of circuit (as shown in Fig. 5), the E.M.F.

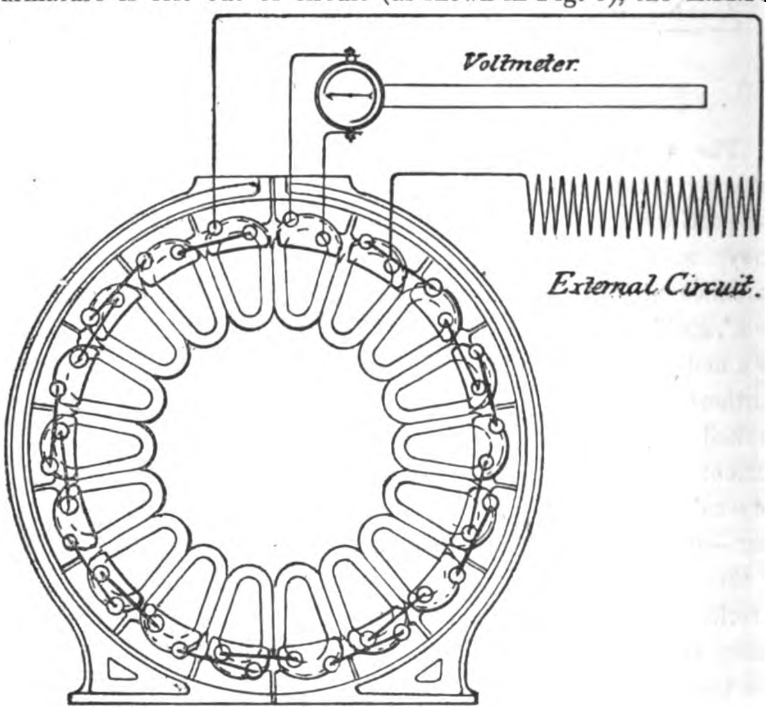


FIG 5.

* *Journal*. vol. xx., p. 236. March, 1891.

of that coil remains constant whatever load may be imposed on other portions of the armature, so long as the speed and excitation remain constant. Mr. Mordey.

I found also that the true load characteristic for the whole armature might be taken by loading one coil only.

This being the case, it follows that an alternator may be fully tested by an engine or motor capable of running only a very small portion of its load. Such a test, although giving all needed electrical information, does not cover the practical considerations that render advisable the long-continued running under the conditions of full load. But it may sometimes prove to be useful.

It follows from this that if a portion of the armature be used as a motor, another portion may be loaded as a generator and the full-load characteristic of the machine obtained, while another portion may be run on a voltmeter only and the open-circuit characteristic ascertained.

I will not occupy further time by alluding to other modifications of this kind. Those I have described suffice to show how useful results may be obtained.

Power-Factor of A.C. Motors.

The "power-factor," or "plant efficiency," of alternate-current motors, whether synchronous or not, is a subject of great importance, on account of its bearing on the economics of power-transmission by alternate currents. Although at the moment the power-factor of motors is not as pressing a matter as the power-factor of transformers (which has received Dr. Fleming's recent attention), it is desirable to find whether for alternate-current motors we have to use cables and machinery capable of carrying a large idle current, or whether we can treat power-transmission projects, so far as these matters are concerned, in the simple manner that is possible with alternate-current transmission with good closed magnetic circuit transformers. I am able to say that, so far as my own observations go, working with one class of alternators, the power-factor of these machines when used as motors under fair loads is very high, if they are used under conditions that are easily obtained.

Mr. Mordey.

Fig. 6 illustrates the way in which the power-factor varies with the excitation. The two V-shaped curves show the current that

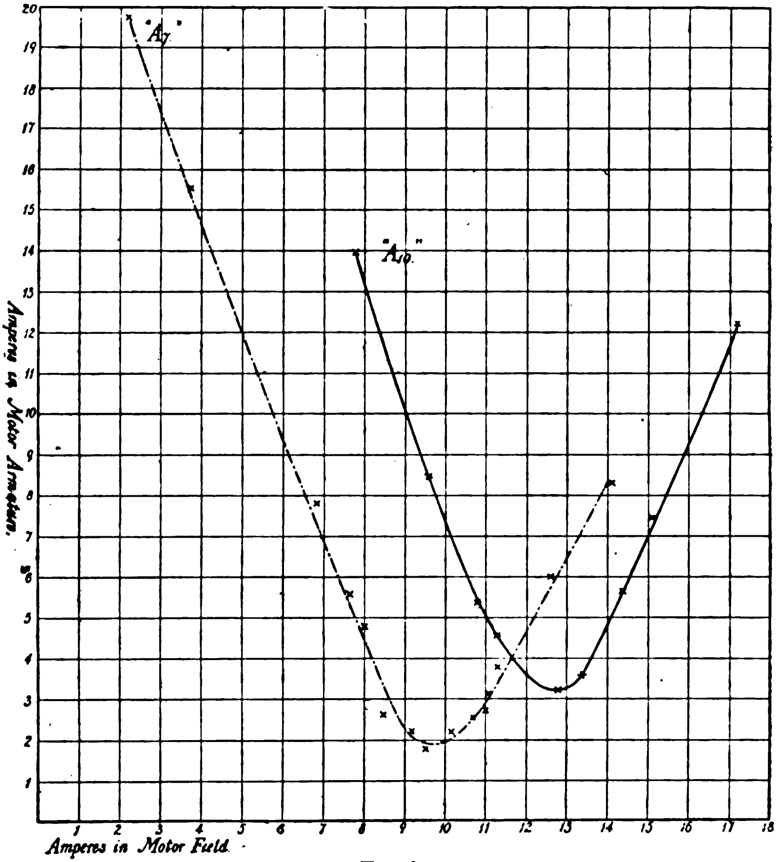


Fig. 6.

was taken respectively by an "A 7" 25-kilowatt alternator, and by an "A 10" 50-kilowatt alternator, running as motors, without load, when the excitation was varied. They were driven by another alternator, and a P.D. of 2,000 volts was maintained across the terminals. Speed constant at 100 $\sim$.

It will be observed that in each case there is an excitation that reduces the armature current to a minimum, and therefore gives a maximum power-factor and maximum efficiency. If the excitation is varied on either side of that value, the armature current rapidly increases.

In order to find whether the same kind of effect takes place when the motor is loaded, one of the motors was made to drive another alternator the load on which was varied. It was found, as was to be expected, that with various loads, and of course at constant speed, there was for each load an excitation that corresponded with a minimum armature current; that the armature current rose on varying the excitation either up or down; and that the excitation that gave the lowest armature current (or the highest efficiency and highest power-factor) was approximately the same for all loads, with the same impressed P.D. Mr. Mordey.

This is what one would expect. With a weak field a large armature current is required to give the necessary torque; while with a strong field the E.M.F. of the motor is higher than that of the generator; the driving current, therefore, is large for a given torque, as it cannot get in advantageously.

It will be remembered that Dr. Hopkinson predicted years ago that an alternator would run as a motor under the condition of the motor having a higher E.M.F. than the generator.*

From these curves we see that the power-factor may vary enormously, but there is no difficulty in making it a minimum and the efficiency a maximum, by always working at the excitation that gives the lowest reading of current.

It will be seen from the table of results given above that the power-factor is high. It appears to fall away at no load to a much lower value, as will be seen from an examination of the two curves. This, however, is of no practical moment, as the machines do not take much to drive them under light load conditions; and although I have not made determinations at intermediate points, it appears that, as was shown by Dr. Fleming to be the case with closed-circuit transformers, the power-factor approached unity as soon as load was put on.

It is satisfactory to find that, in this respect, these two classes of alternate-current apparatus have similar qualities.

---

* *Journal*, vol. xiii., p. 505, Nov., 1884.

Mr. Mordey.

Equalisers.

The present opportunity may be taken to allude to a device which I have used for some time in connection with the working of alternators, and which we apply to all the larger machines. In alternator armatures there are advantages in having the coils arranged in two or more parallel portions, instead of all in series. The insulation of the terminal portions of the winding is easier, and, except in small machines, the conductors are more manageable. But in practice we have found that the parallel arrangement has one drawback. Unless the coils are equally and simultaneously acted on all round, the voltage of the two portions will not be equal, and a circulation of current will take place within the armature. The machine will, in fact, act to some extent as in the dynamotor method described, and a loss of power will result. This has probably been overlooked in machines having revolving armatures, as with such armatures it it is not easy to observe the effect. It is difficult to avoid this circular current by ordinary means. If the pole-faces are exactly alike in size and position, and the polar gap is the same all round, and the armature coils are identical in winding and in relative angular position, the inequality will not exist. But these are conditions that can only be certainly obtained at a considerable expense. With cast magnets the poles vary a little. The armature coils are adjusted radially, and it would be troublesome and expensive to place them so as to comply with the conditions mentioned.

By a simple application of a known property of transformers this difficulty is avoided.

This is shown in Fig. 7. A pair of similar coils is arranged in the manner of a transformer, one coil being placed in each half of the armature circuit. The connections are such that with equal currents from the two portions of the armature there is no magnetism induced, the two windings neutralising one another. But any tendency of one portion of the armature to give a larger current than the other portion is resisted by the "equaliser" (as I term this apparatus), which then acts as a transformer opposing an E.M.F. in the circuit

which has an excess, and adding a corresponding E.M.F. to the circuit of lower voltage. Thus the two portions of the armature Mr. Mordey.

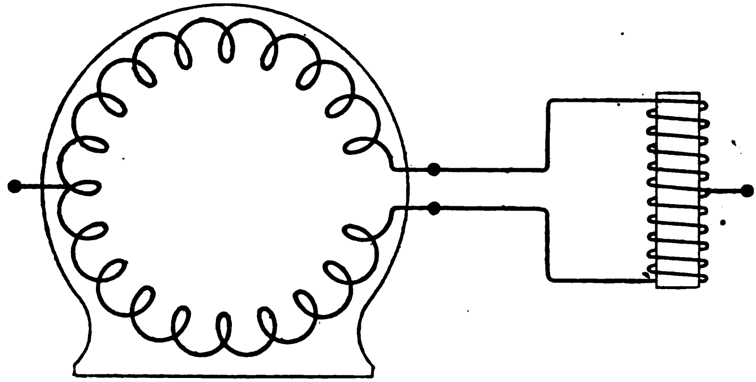


FIG. 7.

contribute equally to the external circuit, while the circular current is prevented, whether on open circuit or under load. The equaliser is quite a small affair. As an example of its efficacy, I may mention that in one case the two sides of a 250-kilowatt 2,000-volt alternator were made so unequal that a current of 16 amperes circulated. The addition of a small "equaliser" reduced this circular current to half an ampere.

I do not know whether this inequality has been noticed by others, but it is well known that it may exist in direct-current dynamos when different parts of the armature are acted upon unequally, and it has been cited as an objection to the parallel winding and connection of armatures in such machines with multipolar fields. Whatever force there may be in this objection, it will be granted that it is not as easily met as in alternators. Indeed, it may be said of alternate-current apparatus generally, that, although difficulties are met with, and often of an unexpected kind, their very nature provides us with new and simple means of avoiding or reducing them.

In iron-cored armatures, whether of alternators or dynamos, the iron core may to some extent tend to counteract inequalities of this sort.

Parallel Working of Alternators.

This subject has received a great deal of attention during the

Mr. Mordey. last few years. I do not desire to add anything to the literature dealing with its theoretical aspects, but, as it may be of interest to some members of this Institution, I take the opportunity of giving a description of the principles on which my colleagues and I act in dealing practically with parallel working.

Some years ago Mr. Raworth and I recognised that this was quite as much a question of prime motors as of electrical apparatus, and we then settled on the main points of the arrangements for parallel working, which have been acted on ever since, practically without change.

1. Each alternator is driven by a separate engine.

2. All the engines are in parallel on the steam pipe, and are supplied with steam at the same pressure.

3. All the alternator fields are magnetised in parallel, and are supplied with current at the same pressure.

4. All the alternator armatures are in parallel, and supply into the mains at the same pressure.

5. Each alternator gives its proper proportion of load by regulation of the amount of steam admitted to its engine.

6. The regulation of potential difference is by the control of the common exciter.

Thus it will be seen that the individual regulation of current (that is, of load) of each alternator is entirely by control of the steam supplied to its engine.

The regulation of the E.M.F. as a whole is by the control of the excitation simultaneously affecting all the alternators.

Each engine must be capable of control when working, either by the stop-valve or by having a governor capable of adjustment when running.

In working alternators in parallel the precautions that should be observed will be best understood by a consideration of the following:—

Consider the case of two alternators.

Assume that one alternator, A, is running and doing work, driven by its engine, E, and that it is required to start the second alternator, A_1, with its engine, E_1.

Start the engine E_1 and excite the alternator A_1.

If the fields of the alternators are excited in parallel from the same exciting source—as is preferred, and as we arrange whenever possible—there is no necessity for any adjustment of the individual excitation (except for differences of temperature). It is merely necessary to connect the field to the exciter. It will then receive the proper field current. Let the double-pole main switch of A_1 be open. The synchroniser is then to be switched on, and the speed of the engine E_1 adjusted till the synchroniser lamp is steady, showing that the alternators A and A_1 are running at the same speed. It will not be found difficult to so adjust the engine E_1 that the synchroniser lamp shall remain steady for several seconds at a time. For greater accuracy a voltmeter may be used in addition to the synchroniser lamp—the lamp for the guidance of the engineman, and the voltmeter for the switchman. When the synchroniser lamp (or voltmeter) is at its highest, the double-pole main switch of A_1 should be closed, so connecting the alternators in parallel. It will be perceived that at this time the engine E_1 is only supplied with steam sufficient to drive the alternator A_1 (without load) at the required speed. The alternators when put in parallel will therefore merely keep in step, A_1 doing no work on the external circuit and receiving no power from that circuit. This will be shown by its ammeter, which will show that no current, or only a very small current, passes between the machines. Mr Mordey

As the supply of steam is increased, the ammeter of A_1 will show an increased reading, while that of A will show a decreased reading, until (in the case of alternators of the same size) the two instruments show that the load is equally divided between the machines. When this condition is attained no further regulation of engines is required, any increase or decrease of load dividing equally between them.

The control of E.M.F. is effected by regulating the exciter, and not by means of resistances in the field circuits of the alternators. As the fields are all wound to excite from the same mains, there is no need for an individual regulation of the fields, except, perhaps, a slight adjustment to compensate for difference of temperature between machines that are being started cold to work with others that have become warm by long running. The

Mr. Mordey. field ammeter should be observed in order to give each machine the same excitation.

To stop one alternator when two or more are running, the load should not be switched off suddenly, but steam should be gradually reduced by the governor or stop-valve of the engine until the alternator is doing no work, as shown by its ammeter. Then the switch between the alternator and mains should be opened and the engine stopped.

If the steam is gradually reduced in this way, the load will be transferred to the remaining engines and alternators before the machine is switched off, and no racing of the engine can occur.

Steam should be turned off till the ammeter needle goes back to zero, or nearly zero, showing that the alternator is not doing any work. If steam is continued to be turned off beyond this point, the alternator will then *take* current (to drive it as a motor and to enable it to drive its engine), and the ammeter needle will, after going back to zero, show a deflection representing this received current.

It may perhaps be pointed out that artificial loads need not be used in putting alternators in parallel. This is sometimes done, but it can only be necessary where the characteristics of the machines show great variations between no load and full load.

In conclusion, I have to thank the Brush Company for the preparation of the diagrams; and my hearty acknowledgments are due to Mr. Woof and Mr. Watson, two of the company's heads of works departments, and to Messrs. Stobart, Hansard, and Foyster, students and assistants, for much careful and willing assistance in carrying out trials by these methods.

The PRESIDENT: I will ask Professor Ayrton to kindly commence the discussion on Mr. Mordey's paper.

Professor AYRTON: Since Mr. Mordey was kind enough to describe his method to me some months ago, some experiments have been conducted at the Central Institution in extension of his method. I would ask that Mr. Miller, one of the students, might describe what his group of students have been doing.

Mr. Miller Mr. LESLIE MILLER: Some months ago Mr. Mordey described to Professor Ayrton the ingenious method which he was employing

for measuring the power and efficiency of an alternator, and which Mr. Miller. has formed the subject of to-night's paper. This method, which consists in splitting up the armature and using one half as a dynamo and the other half as a motor, appeared at first sight to be only practicable when the armature was stationary, and the field magnet rotating. But it subsequently occurred to Professor Ayrton and Dr. Sumpner that the principle of using one half of an alternator as a dynamo and the other half as a motor was as applicable to a rotating armature and many pole stationary field as to a rotating field and a many coil stationary armature. For if the stationary field magnet be electrically divided into two halves, and the connections of the one half be reversed, then, without any other change in the rotating armature, one half of the machine acts as an alternating motor, while the other remains a generator. Towards the end of last year Messrs. Fleming, Nicholson, and myself, carried out a series of experiments on the 12-H.P. Ferranti alternator at the City and Guilds Central Institution, and the results confirmed the idea that this method of testing the power and efficiency of an alternator with rotating armature could be practically employed. The method we employed was briefly as follows:—The field magnet coils of the Ferranti dynamo were connected in parallel, so that the current passed round the upper and lower coils in opposite directions. The Ferranti alternator was driven by a separately excited direct current motor, the shafts of the two machines being joined by a spring. Throughout the whole experiment the currents in the two halves of the Ferranti field were so adjusted by means of a variable resistance in one of them as to produce sufficient E.M.F. in the armature (which was short-circuited by an alternate current ammeter) to send 20 amperes through this armature. The alternator was now run at different measured speeds, and the electrical power required to drive the direct current motor measured; then, subtracting the power wasted in heating the armatures of the Ferranti and the direct current motor, we obtained the power expended on eddy currents and in overcoming the friction of bearings of the dynamo and motor. The power wasted in the motor was then separately determined

Mr. Miller. by ascertaining the power required to drive the motor at the same speed when its spindle had been uncoupled from that of the alternator. So that we have finally the power wasted in the Ferranti alternator in eddy currents and in friction. Having obtained this, it was then quite easy to calculate the efficiency at different speeds by aid of the characteristic curves of the Ferranti alternator.

Professor Ayrton. Professor AYRTON: It will be seen that our method, just described by my student Mr. Miller, of testing an alternator with a rotating armature by dividing the field into two parts is equally applicable to direct current multipolar machines. Direct current dynamos are now being made on the continent with many poles, not merely with the four or six poles we use in England. In fact, the machines on the continent are not so very unlike an alternator, only the currents are commutated. Such a machine, then, can be tested at once by simply dividing the field-magnet circuit electrically into two parts, and reversing the connections of one part relatively to those of the other. It is not necessary, as Mr. Mordey suggested, to have two commutators on the armature; the armature may be an ordinary one with a single commutator. All that need be done is to connect one portion of the field magnets the wrong way, so to say, to the remainder of the field magnets, and rotate the machine by means of a very small direct-current motor.

In addition to supplying a direct current to drive this motor, it would be necessary to supply current to excite the field of the dynamo under test, since with the field magnets joined up as described the machine would not excite itself. And the power required to send these two currents represents, after allowing for the waste of power in the small driving motor, the power that must be supplied to the dynamo under test to cause it to develop the current in its rotating armature. Subtracting then the power developed by the armature from the power already referred to we have the power wasted in excitation, in mechanical friction, and in eddy currents, and then the efficiency of the dynamo can be at once calculated.

It might be objected that the method I have spoken of, using

Professor Ayrton.

direct-current dynamos with multipolar fields, coupling one half (the wrong half) to the other half, would produce a distortion of the field, and the result would not truly represent what would happen if there were no such distortion. That is true to some extent, but it is equally true with Mr. Mordey's method; in one half of his armature the current is in one direction, while in the other it is in the opposite direction. The method he suggested for testing the motor-dynamo would have the same sort of objection. However, I do not think that with multipolar field that is a very serious matter. If you only had two poles no doubt it would be so serious as to render the method wholly inapplicable. You would distort the whole of the lines of force with a two-pole dynamo. But if there are many poles in the circle round the machine, whether the machine produces an alternating or a direct current, not much error will be introduced in the test by having one half of the set of magnets magnetised the other way.

Mr. Mordey has referred to Dr. Sumpner's application to the testing of transformers of Dr. Hopkinson's coupling of a dynamo and motor method of testing. The method described by Mr. Mordey to-night on the method of testing an alternator, which we have employed at the Central Institution, and which has been described by Mr. Miller is the exact analogue of a method we used last spring for testing an equal ratio transformer—that is, a transformer which transformed from 100 volts to 100 volts. In that case, as we pointed out, there was no necessity to employ two transformers at all, but to couple up the primary and secondary coils of this equal ratio transformer to the mains, so that while the primary coil took power from the mains the secondary coil returned power to them. And that was the method we employed and described in our B.A. paper of last year, for testing the efficiency of an equal ratio transformer. In such a case a large transformer can be tested by means of a small alternator without it being necessary to use a second similar transformer.

Of course there is one point about all these methods, one objection which does not apply to the original method suggested by Dr. Hopkinson some years ago. In Dr. Hopkinson's

Professor Ayrton.

method there were two distinct direct-current machines with their shafts coupled together, one working as a motor and one as a dynamo, and the H.P. that each machine was developing was actually transmitted through the shaft. Now, it is to be observed in the interesting method which Mr. Mordey has described, and the extension of it described by Mr. Miller, that the stress on the shaft is not tested at all, and it might be that the shaft was weak, or the bearings might heat when the shaft was required to transmit 100 H.P., and this would not come out at all with the methods of testing described. There is this, however, to be said in favour of the method, viz., that it might be possible at some time or other to drive the machine and see that you could use it to produce something like 100 H.P. without measuring the actual efficiency, and still leave Mr. Mordey's method valuable for efficiency testing. It is quite clear, however, that if you have not an engine giving more than 10-H.P. you cannot in any way transmit 100 H.P. through the shaft, and therefore the shaft and the bearings must remain untested, although the efficiency of the machine at full load may be ascertained fairly accurately.

Mr. Mordey has touched on a very interesting point—the circling of the current in the coils of an alternator joined up in parallel when the E.M.F. of one coil is not exactly the same as the E.M.F. of the coil in parallel with it. My students have noticed the same thing with a transformer, the secondary winding of which consists of two coils joined up in parallel. Some time ago Professor Perry pointed out that if one portion of a transformer, whether the primary or the secondary winding, consisted of two coils in parallel, there was danger of circling of the current in the two parallel coils, if the number of convolutions in the one were not exactly the same as the number of convolutions in the other. But he did not observe that, even if the numbers of convolutions on the two coils were identical, there would still be circling of the current if the magnetic leakage were different for these two coils, that is to say, the current flowing in the one winding would be far greater than the current flowing in the other.

We have this evening heard Mr. Mordey read an extremely interesting paper, interesting from a variety of reasons: interesting first because it proposes a new method of testing alternators, which enables a large alternator to be tested with the supply of a very small amount of power, but especially interesting, I think, because it contains such valuable detailed rules for the parallel working of alternators. It has been urged by those who are in favour of direct-current working that it is impossible to work alternators successfully in parallel; with these instructions, however, which Mr. Mordey has drawn up so fully, it would seem that that difficulty is got over, and that, therefore, the argument, perhaps the only remaining argument that could be alleged against alternators for electric lighting, that they could not be worked in parallel, has now disappeared. Professor Ayrton.

The PRESIDENT: Is there any member present who could not be here next meeting and who would like to make some remarks on the paper?

Professor S. P. THOMPSON: We have once more to congratulate Mr. Mordey on a thoroughly happy paper, one that shows that he is not afraid of putting theoretical considerations before us, even though he claims to put them before us under the guise of purely practical matters of experience. We have two or three points before us in this paper that perhaps call for a little more than passing comment. For example, to go to one of the last things first, that very curious and interesting diagram of the current required in the field of the motor at different stages of the excitation, brings home to us, I think, in a way that nothing else would do the meaning of Dr. Hopkinson's prediction that you could run a motor with a higher E.M.F. than the generator that was feeding it. For it is obvious that if you had a motor with a higher E.M.F. than the generator that was feeding it, you could not possibly work those two machines in absolute opposition of phase. They must get somehow into a different phase relation not in complete opposition, and that can only be done by a current passing greater than the minimum current. One may, in fact, calculate from the ratio between the minimum current and the current passing at any particular phase the angle of phase Professor Thompson

Professor Thompson

difference between the machine as run and the machine as it would run if the current was the minimum. This is a matter of practical importance too, because it is obvious that motors ought always to be so run that the current running up and down the mains shall be a minimum, that we may waste as little power in the mains as possible. These are exactly the same considerations as those which were brought so prominently before us by Dr. Fleming, in pointing out the disadvantage in a circuit of transformers that require considerable magnetising current.

When Mr. Mordey divides the armature into two parts, as in Fig. 1, or any of the others, and runs the machines thus, that surely is not quite exactly the equivalent of running a generator and a motor, because the generator and the motor are not rigidly coupled together so that the E.M.F.s are necessarily in entire opposition of phase, for if there is any self-induction in the circuit in between, or if there is any capacity on the circuit in between, then there will be a difference of phase between the two machines, that is to say, they will not be exactly in opposition of phase as they are in the motor coils and the generator coils in the divided machines. This may be entirely unimportant—at any rate, when one works with low voltages and short mains—though with long mains and high voltages it may make an important difference. Again, Mr. Mordey makes the assumption that the efficiency of the machine as a motor is the same as when used as a generator, an assumption which he says is fair. Well, again, that depends to some extent on the conditions of the circuit. The circuit is one that has self-induction in it, or in which capacity effects are important, then it is a question whether the assumption is fair, because the phase relation of the current may be different, and the current may be acting to excite its own field magnets in the latter case. On the other hand, the current in the armature of the generator might be demagnetising its own field magnets. The effects might not be important, still they exist. Again, on page , Mr. Mordey says: "Running at full current and small power may also be done by using simple choking coils for the external circuit, but this introduces disturbing influences which make it of very little use." But those are exactly the

disturbing influences that do come in in running. When running during three quarters of the day there is little or no current in the secondaries of the transformers in the network, and their primaries are simply acting as choking coils. Hence, light load running does in practice introduce those very disturbing influences of self-induction which he says he would not introduce in these experiments. I would make here a similar criticism to that which I would have made on Dr. Fleming's paper if I had had the opportunity. It was a very valuable paper, but the conditions under which the tests were made, excellent as they were for giving us something like a distinct view of the subject, were really not those of actual practice, because in actual practice you do not keep the primary volts a constant thing, but you keep the secondary volts constant. Further, if there are motors in the consumers' main, then the phase relations in the secondary circuit are not simple, as they were assumed by Dr. Fleming. It simplifies the matter no doubt to assume self-induction to be absent, but it is not so in reality, because in reality you have self-induction in the choking coils of the primaries. Professor Thompson.

Some years ago there were alternating current machines brought into the market, mainly, I think, by Messrs. Siemens Brothers, that worked at a very low voltage, 40 or 50 volts. Of course such machines have no *raison d'être* for modern working. These machines have, of course, a large number of circuits in parallel, and it was at first a source of wonder to me how much power these machines contrived to absorb when running light and doing no work. At first I assumed it was due to eddy currents in the clamps holding the coils together. Then I found those clamps did not heat, and I could not account for it on that assumption. I came to the conclusion, then, that the cause of that very low efficiency, that relatively enormous waste of power, at light load and at no load, was simply the cross connections in parallel of the different coils. The coils themselves not being equal among themselves, or the pairs of poles not being of equal power, therefore the internal currents running in and out the cross connections of the armature waste the power given to the machine.

The PRESIDENT: As this is an intensely interesting subject, and one of great novelty, I propose to adjourn the discussion until our next meeting on March 9th.

Foreign Members:

Hayazuchi Kodama, M.E.	Junsuke Miyake, M.E.

Associates:

Algernon Burton.	Arnold Greaves Hansard.
T. Cooper, M.A.	Francis Ince.
William John Croft.	Arthur Jackson.
Howard Field.	Francis Miller, B.A.
Evan W. H. Fyers, B.A.	Leveson Scarth.
Herbert E. Hall.	Thomas Wardell.

Students:

Charles Herbert Archer.	James Arthur Bernard Horsley.
Arthur George Bird.	John Chichester C. Lanndon.
John H. Bunting.	Carl Adolph Louis Prusmann.
Arthur Clifford.	Cyril Stromeyer.

Malcolm Warner.

The meeting then adjourned.

ORIGINAL COMMUNICATIONS.

"NOTES ON A SELENIUM CELL,"

By E. O. WALKER, C.I.E., Member.

The selenium cell used in the following experiments was very kindly supplied to me by Mr. Shelford Bidwell, through the goodness of Mr. Preece, who had made known my want. The experiments were undertaken in view to ascertain whether there was sufficient variation in the strength of sunlight at certain hours of the day to account for fluctuations in earth-currents on the supposition that they were due to the electro-magnetic action of light. This hypothesis is not supported by the results of the experiments, since by the action on selenium, a continual decrease in resistance is witnessed beyond the time when the maximum strength of earth-current usually takes place. Thus during the time over which these experiments extended, namely, from 7 to 10 a.m., the earth-current has usually had a maximum from 9 to 9.30 a.m., and from data received through Mr. C. Chambers, F.R.S., of the Colaba Observatory, the maximum easterly variation of the needle for the first six days of July occurred about 7.40 a.m. By the action on selenium, the red rays were shown to be steadily growing in strength from sunrise up to and beyond the time when the observations ceased, about 10 a.m. Leaving these considerations aside for the present, it is thought to be of interest to bring to notice the behaviour of the selenium under the combined action of heat and light in the direct rays of the sun, and also under that of reflected sunlight.

The observations were taken in Madras, and the variations noticeable in the resistance of the selenium as displayed by the readings of the galvanometer were due to passing clouds, to dust haze, and to occasional light winds. A reflecting galvanometer was employed, with 1-9th shunt and one Minotto cell. The constant was taken before and after each day's experiments. Excluding the selenium cell, the conditions of the circuit were

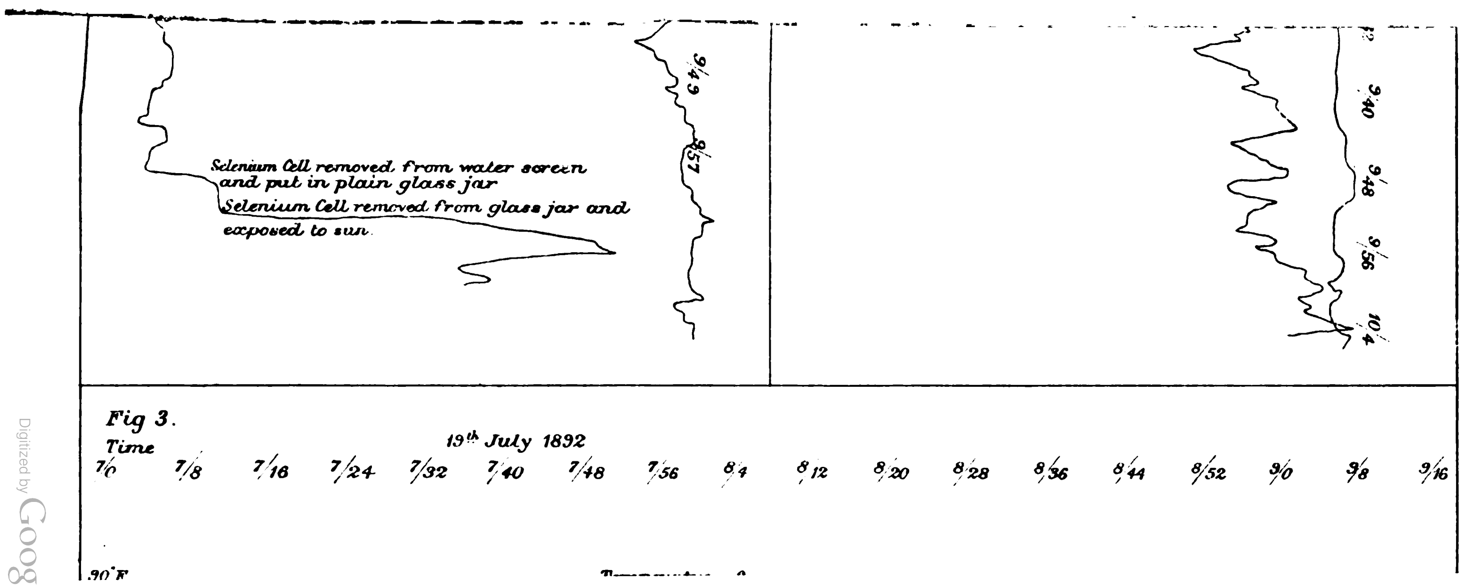
practically constant. For temperature observations a Casella thermometer was used.

The resistance of the selenium cell at a temperature of 89° Fahr. and within a room, was found to be 0·91 megohm.

The observations plotted in Fig. I. are those of the selenium cell exposed to the direct rays of the sun, permitted to fall perpendicularly on the face of the cell. Thus the effects are of light and heat combined, and those of heat preponderate, as they seem from the succeeding plates always to do, when the direct rays act upon the selenium. With the temperature at 118° Fahr. in the sun, when the cell was shaded for two seconds, the deflection of the galvanometer fell from 110° to 60°, showing that the resistance of the selenium cell had nearly doubled. The substance appears to respond very quickly to heat as well as to light.

In Fig. II. are shown the results when the selenium was exposed to the direct rays of the sun, but screened inside a glass jar placed within a jacket of water in another jar. The heat rays were thus largely, but not entirely, cut off. At 9·36 a.m., as will be seen on the plate, the sun had risen sufficiently high to be reflected on the surface of the water forming the jacket and a dark band was thus suffered unintentionally to be cast upon the surface of the selenium, with the effect of a rise in its resistance. At 9.59 a.m., the outer jar and the water jacket were removed. A considerable fall in resistance is thus noticeable, followed by an enormous fall when the selenium cell was removed from the second jar and exposed directly to the sun.

It seemed preferable, in order to obtain effects of light as nearly as possible dissociated from those of heat, to expose the cell to reflected light only. It was therefore placed inside a room, where the temperature remained fairly constant, in the shade below an open window, with the face of the cell turned towards the interior of the room. In this position the direct rays of the sun were reflected upon the cell from a white wall six feet distant. The thermometer was placed similarly to the cell and three feet distant from it. At a less distance it was found that the heat of the body of the observer affected the cell. The temperature of the room varied very little in these observations,


9/49
9/57
Selenium Cell removed from water screen and put in plain glass jar
Selenium Cell removed from glass jar and exposed to sun.
9/40
9/48
9/56
10/4
Fig 3.
Time
19th July 1892
7/0
7/8
7/16
7/24
7/32
7/40
7/48
7/56
8/4
8/12
8/20
8/28
8/36
8/44
8/52
9/0
9/8
9/16
90°F

and the alterations in resistance of the selenium were due almost solely to the reflected light. In this there was frequent change, due to light vapours and dust obscuring the sun. The following figures will show to what degree the activity of the sunlight was at that time in the morning increasing. At 7.39 and 8.29 a.m. the sun was bright; the readings of the galvanometer at these times were respectively 54½° and 65°: the temperature was the same in each case. Calculating from the constant of the galvanometer, the resistance of the selenium at 7.39 a.m. was 0·835 megohm, and at 8.29 a.m. 0·7 megohm. Thus, in fifty minutes the brightening light had caused a fall in resistance of 0·135 megohm, or about one-seventh of the normal resistance of the selenium at the temperature then prevailing. It was found that the body of the observer would, if brought to a distance of a yard from the cell, cause an equal change by radiated heat. A screen of white cloth interposed between the two caused the selenium to return to its former condition; but with no screen the influence of the body was noticeable two or three yards away. It is not impossible, therefore, that selenium may be of use for delicate thermometrical purposes. As a medical aid in treatment of fevers it might be of value where the indications of the galvanometer must be continuously recorded.

The method of observation employed in these investigations affords means of measuring the rate of change of activity of sunlight, and ought to be a useful one in meteorological observations. For example, referring to Fig. III., it will be seen that from 7.39 to 9.14 a.m. the current in circuit had increased under the action of light by nearly one-half, or by 0·4, making a deduction of 4° from the reading at 9.14 a.m. for a rise of temperature of 1° F., which was found to be an average value. This increase was equivalent to an access of electro-motive force in circuit of 0·4 volt. The dimensions of the face of the selenium were 4·17 by 1·85 centimetres, its area therefore 7·71 square centimetres; thus the rate of increase for the period above stated of the force affecting the condition of this substance was per square centimetre equivalent to about 0·05 volt, or 5×10^6 C g.s. units.

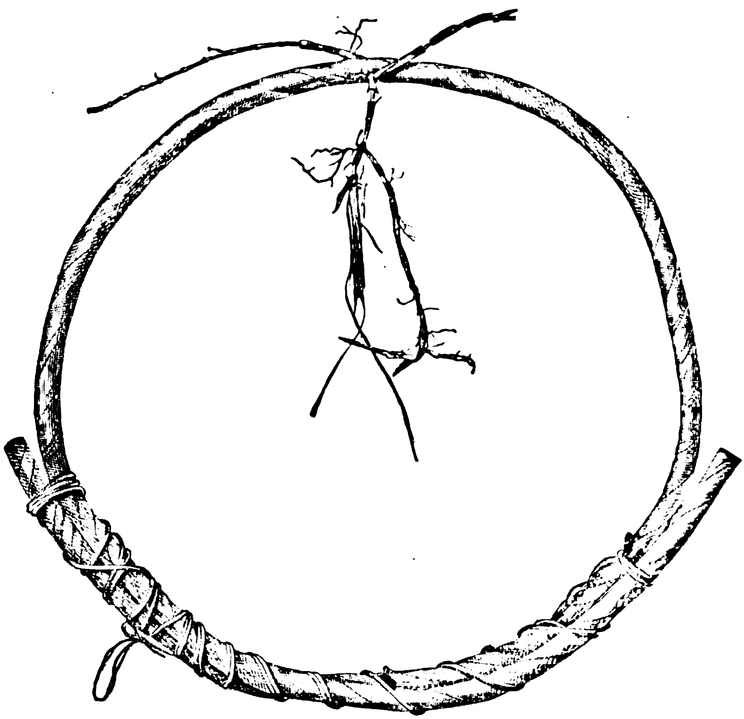
NOTE.—Mr. J. Alphonso, of the Indian Telegraph Department, assisted in the above observations.

CALCUTTA,
1st *February*, 1893.

DEAR SIR,—

As illustrating the dangers to which underground wires are exposed in India, I send you a photograph showing how a root of grass has grown right through a piece of india-rubber cable core (Hooper's Core).

It happened in this way. The piece of core in question, after being treated with a composition said to be a preservative



against white ants, was buried, unprotected by any armour, in soft damp soil at Calcutta for six months to see what happened. On the piece of core being dug up, the grass was found to have grown through it as shown in the photograph.

What at first sight appears to be an impossible performance on the part of grass seems less so when you come to examine

the hard, sharp, needle-like points which characterise the roots of this species of grass. I enclose some of them for inspection.

Yours faithfully,

P. V. LUKE, M.I.E.E.

To the SECRETARY,
INSTITUTION OF ELECTRICAL
ENGINEERS.

ABSTRACTS.

M. VON FREY.—ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY.

(*Beiblätter*, *No.* 4, 1892.)

The author has investigated this question by using Kohlrausch's method for the measurement of the resistance of electrolytes by means of a telephone and alternating currents. It was found necessary to somewhat modify the method: the metre measuring-wire had so small a resistance compared with the human body, that it had to be replaced by a liquid resistance of zinc sulphate solution in a trough, in which the telephone terminal could be moved. It was found also that comparatively large electrodes had to be used, platinum electrodes of 10 to 20 square centimetres surface were found to be quite inadequate, and it was only when the contact surfaces were increased to several hundred square centimetres tha the position of least sound could be determined with any degree of accuracy; it was also found that the amount of surface required varied with the region of the body. When large surfaces of the body were uniformly moistened by immersion in liquid into which the current was passed by zinc plates, the resistance of the body was found to be comparatively small, being about 300 or 400 ohms from one hand to the other.

The author concludes that it is probable that polarisation takes place mainly, if not entirely, in the upper layers of the skin.

P. DRUDE.—MAGNETO-OPTICAL PHENOMENA.

(*Wiedemann's Annalen*, *Vol.* 47, 1892, *No.* 7, *p.* 353.)

This is a long and elaborate mathematical and experimental investigation, the results of which are summed up by the author as follows:—

1. The systems of explanation of the magneto-optical phenomena of metals hitherto advanced, which introduce only one single magneto-optical constant, do not agree with the results of experiment.

2. The author advances a new system of explanation, which agrees well with the observations on reflected light, and which gives a magneto-optical constant of almost the same size as that calculated from the observations on transmitted light.

3. This system may be considered as an amplification of the equations to which the electro-magnetic theory of light leads.

L. ZEHNDER.—MAKING THE HERTZ SPARK EFFECT APPARENT TO AN AUDIENCE.

(*Wiedemann's Annalen*, *Vol.* 47, 1892, *No.* 9, *p.* 77.)

The author has endeavoured to attain his object by causing the Hertz spark to

start the discharge of a high-tension accumulator in an exhausted tube. In his first experiments the spark gap was in series with the tube, but outside it. The spark gap was so regulated that when the battery was off, small sparks jumped across the spark gap, and it was found that if the battery was just too weak to start a discharge through the vacuum tube and the spark gap, the breaking down of the resistance of the gap by the Hertz spark was enough to start the accumulator discharge.

The spark gap was next put inside the vacuum tube, in the line of the discharge, and nearer the cathode, as the greatest resistance is known to be near this spot. It was then found that up to a certain critical distance apart of the spark wires, the usual bright spark took place, but beyond this distance a glow took its place. It was found that only in the case of its taking the form of this glow could the Hertz spark start the battery discharge.

The final arrangement arrived at by the author is to adjust the current and P.D. of the battery by means of adjustable resistances, formed of tubes containing a solution of cadmium iodide in amyl alcohol, in shunt and in series with the cell. The spark gap is in the vacuum tube between the electrodes, and the battery can be so adjusted by means of the resistances that the discharge starts and stops with the Hertz spark. This discharge can be seen in a fairly lighted room 10 metres away.

The author shows that an induction coil is not so suitable for these experiments as a battery as a source of discharge current; it may, however, be used if a battery be not available. The battery used consisted of 600 small Planté cells.

A. BANTI—THE MAGNETISATION OF NICKEL UNDER VARIOUS CONDITIONS OF MECHANICAL STRAIN.

(*Beiblätter, Vol.* 16, 1892, *No.* 8, *p.* 556.)

Nagaoka observed that a body which is subjected to a certain torsion, and is simultaneously subjected to a suitable critical tension, and to a given magnetising force, can reverse its polarity. The author soldered nickel wires, 30 cm. in length and 1·2 mm. in diameter, to brass cylinders, the upper one fixed and the lower one carrying a scalepan on a hook. The lower cylinder passes through two tubes, one placed inside the other; the outer is fixed, and the inner capable of rotation by an amount shown by an indicating pointer, and of being clamped in any given position. A magnetising coil, 48·1 cm. long, surrounded the nickel wire, at the lower end of which is a magnetometer of the usual type.

The following are the more important results:—

If a nickel wire in a given magnetic field is subjected to a given strain, and then twisted and untwisted, the sign of the magnetism changes with the torsion-cycle. There appears to be no definite relation between the strength of field and the turning weights at which the reversal takes place. If the wire be a new one a greater load is required for a given magnetising force to produce the reversals than if the wire be one which has been long in use. The latter becomes more strongly magnetised, and the variations of induction for a certain torsion cycle are greater.

The change of sign of the magnetism arises from a true reversal of the polarity

of the nickel wire. Supposing that with a positive torsion of 0°—180° the magnetism of one end grows in the positive sense, then it grows in the negative sense at the other end, and similarly at symmetrical points on the two halves of the wire. At the critical angles of torsion the wire is everywhere non-magnetic. The initial elastic state of the wire has a considerable influence on later results. The torsion is the most important and sufficient condition for the phenomena; the pull makes the working of the torsion quicker, but it is neither necessary nor sufficient in itself to produce the effect.

HERMANN MÜLLER—THE USE OF ACCUMULATOR—SUB-STATIONS IN ELECTRIC LIGHTING.

(*Electrotechnische Zeitschrift, Vol.* 13, *No.* 28, *p.* 378.)

The author considers that in sub-stations of this kind three points should be observed: that each cell should be perfectly accessible; that therefore the arrangement must be systematic; and, lastly, that the ventilation should be perfect, and any instruments or apparatus thoroughly cut off from the cell-sheds. He proceeds to describe a station in Düsseldorf—one of three—which is to contain two batteries of 140 double cells arranged on a three-wire system, of which one is now installed, having an output of 500 amperes. The batteries are arranged in the usual manner —to be capable of charging and discharging in parallel. The maximum pressure being 2·6 volts per cell the discharge switches have to be capable of cutting out 28 cells, in order to keep the supply at 110 volts during maximum charge. These sets of 28 cells are arranged over one another, in order to have the copper connections of simple character.

F. UPPENBORN—THE LAUFFEN-FRANKFORT TRANSMISSION.

(*Electrotechnische Zeitschrift, Vol.* 13, *No.* 28, *p.* 379.)

The distance bridged by this transmission was 175 kilometres—about 107 miles. The Portland cement works at Lauffen supply water power of about 1,500 H.P., of which 600 H.P. are used in the work, and 900 H.P. are available for lighting and power transmission, chiefly for the town of Heilbronn. One of the three turbines used for this purpose was utilised for the transmission by three-phase current to Frankfort, and drove a 300-H.P. dynamo, giving 50 volts and 1,400 amperes at a speed of 150 revolutions per minute. The machine, built by the Oerlikon Company, has fixed armature, and rotating field magnet of 32 poles, arranged alternately north and south; there is only one magnetising coil, arranged round the periphery of the wheel-shaped magnet, whose flanges, suitably prolonged, form the poles. The whole magnet is overhung. The current is taken from the dynamo to a switchboard, and thence to two 100 kilowatt transformers of three-phase type, which are insulated by means of oil. The pressure is here transformed up in the ratio 1 : 123, and is led into the three conductors which are carried on porcelain oil insulators and are bare. Their diameter is 4 mm., and their total weight was about 60,000 kilogrammes. In Frankfort the current is transformed down again. Half the large current so

obtained was utilised on a bank of lamps in the Exhibition, and the remainder for motors. Extensive tests were made of the efficiency of transmission, which was found to be 75 per cent., the losses being made up of the following components: 8 per cent. in the dynamo, 11 per cent. in the main conductors, and 3·4 per cent. in the transformers.

THE TIVOLI STATION FOR THE ELECTRIC LIGHTING OF ROME.

(*Electrotechnische Zeitschrift*, 1892, *No.* 37, *p.* 500.)

This is a transmission which exhibits several features of interest. The well-known waterfalls of Tivoli supply the power, the amount of water passing being about 40 cubic feet per second, and the fall altogether about 330 feet. The main channel feeds three pipes, from each of which three channels lead to as many turbines of the Girard type. Each group of three machines consists of two 330-H.P. turbines and one of 50 H.P., which are coupled directly to the corresponding dynamos, and which run at 170 and 375 revolutions per minute respectively, being governed by automatic regulators on the Ganz system, which are said by controlling the amount of water supplied to regulate quite as well as the best steam engine. All this apparatus is underground, and every precaution is taken to keep the machine room dry. The dynamos coupled to the larger turbines are alternatars giving 42 amperes at 5,100 volts, and having wheel-shaped field magnets about 7 feet in diameter, and having 30 poles. The smaller turbines run the exciters, four-pole machines giving 180 volts and 150 amperes, and each capable of exciting three alternators. Both alternators and exciters are run in parallel, an artificial load being provided to run the machines up to speed upon. Automatic regulators of the Bláthy type keep the pressure in Rome constant by altering the excitation of the direct-current machines. The switching arrangements are as usual, but the switches are of unusual type, consisting of ebonite pots containing mercury, into which contact rods are plunged by raising or lowering the pot. The distance of transmission at high tension is 26 kilometres, or about 17 miles, bridged by four conductors run on oil insulators, three of which are capable of carrying full load. These end in a transformer station near the Porta Pia, just outside the walls of Rome, where the pressure is about 4,000 volts (as there is 20 per cent. drop at full load), and where it is lowered by two groups of 16 transformers to 2,000 volts, and fed into the cable network in Rome. There are also transformers feeding the 250 arc lamps now installed, and capable of supplying 600. These have special windings to allow of various numbers of lamps being run in series. The whole system of distribution is that of Zipernowski-Déri-Bláthy, and was carried out by Ganz & Co., of Budapest.

G. HERHOLZ—ON EARTH-PLATES FOR LIGHTNING CONDUCTORS.

(*E. T. Z.*, 1892, *No.* 32, *p.* 430.)

This paper contains some remarks on experiments on the subject carried out in Germany for the military authorities and the Post Office, which were chiefly directed to the resistance of the earth connections, and the form, material, and bed of the earth-plate itself.

Referring first to powder magazines, the author says that the commission appointed by the military authorities had found that a large network of conductor was required on a roof, earthed at several points. The earth-plates were metal tubes about 4 in. in diameter, sunk into the earth so as, if possible, to reach the water-bearing strata. The evidence before the commission went to show that the resistance of the earth connection should not exceed 10 ohms, but experiments tended to show that an average value of something like 25 ohms was more probable with such earth-plates. Further experiments were then undertaken with the results given in the following table:—

No.	Form of Electrode.	Metal.	Size (sq. ft.).	Bed.	Resistance Ohms.
1	Tube	Iron	10·8	In water ...	6·3
2	,, ...	,, zinc-plated ...	,,	,, ...	6·7
3	,, ...	,, tin-plated... ...	,,	,, ...	6·1
4	,, ...	Copper	,,	,, ...	6·25
5	,, ...	,, tinned... ...	,,	,, ...	5·5
6	,, ...	Lead	,,	,, ...	9·2
7	Plate...	Copper	,,	,, ...	14·0
8	Wire network ...	,, ...	,,	,, ...	16·2
9	Tube...	Iron	5·4	Coke	10·25
10	,, ...	,, ...	,,	Dry earth...	83·0
11	Wire network ...	Copper	10·8	Coke	22·0
12	,, ,, ...	,, ...	,,	Dry earth...	65·5
13	,, ,,	Copper, with covering of iron tape	,,	Water ...	16·0

The author remarks that these results (1 to 5) show no particular difference as between iron and copper; lead, on the other hand, being bad. Zinc covering or tinning does not affect the resistance, but (with iron) affects the durability; the author does not recommend zinc, as its adherence to iron is not very great. As regards form, the tube form is much better than the others and is much more easily constructed and laid. The coke-bed (which would prove useful where the water cannot be reached) is made of finely powdered material, well pressed down to make good contact. The iron-covered copper was used with the idea that the iron would prevent oxidisation of the copper, and that the iron salts produced might permeate the earth round and cause a better contact. The experiments had not been in action a sufficient time to determine the correctness or otherwise of this assumption.

DR. MAX CORSEPIUS—THE LOSS OF ENERGY BY HYSTERESIS IN THE ARMATURE OF DYNAMO MACHINES.

(*E. T. Z.*, 1892, *No.* 33, *p.* 443.)

This article is one in which the author works out a mathematical formula for the loss, expressed in terms of the output of the dynamo, the number of poles, the

armature current, and the field, starting from the formula lately given by Steinmetz—

$$W = K B^{1 \cdot 6},$$

for the loss of power by hysteresis in iron, and B. being the induction.

Assuming this formula, the author finds for the total loss in the armature from this source (in watts)

$$W = \frac{65 \cdot 5}{10^6} a \sqrt{\frac{p J}{\beta}} E H^{0 \cdot 6}.$$

Where a is the ratio of the diameter of the covered wire to the bare, p is the number of poles, β the current in the armature wire, J and E the current and E.M.F. of the dynamo, and H the intensity of the field, the assumptions being that there is only one layer of wire on the armature, that with four pole machines the thickness and width of the iron ring are given as a function of the diameter of the same, and that the periphery, divided by the number of poles, is a fixed quantity. As variables are left, the electrical efficiency, the armature current in each wire, the thickness of the insulation of it, the strength of field, and the number of poles.

The formula the author finds to be very instructive, and to lead to a solution of all hysteresis problems in dynamo design. He points out that the value of "K" in Steinmetz's formula has been taken at 0·002, and the constant must be altered accordingly for other qualities of iron.

He comes to the following conclusions ;—

For a given *output* of dynamo, the loss increases as the square root of the number of poles, and decreases as the square root of the load. Besides these, it increases with the field strength or magnetisation in a proportion somewhat greater than the square root of the field.

The hysteresis loss does not depend on the electrical efficiency. Again, suppose a series of similar machines for certain electromotive force, the loss in the armature grows in proportion to the square root of the output. That is, it is easier to make large machines efficient than small ones. The author points out also that if the electrical efficiency be altered, with a consequent alteration of dimensions and speed for the same output, the hysteresis loss remains the same; and he finally concludes that the limits of the values of the various factors depend upon such considerations as sparking, speed, and danger to the armature by over heating—all of which will necessarily be taken into account by the designer.

C. P. STEINMETZ—THE LAW OF MAGNETIC HYSTERESIS, AND ALLIED PHENOMENA.

(*E. T. Z.*, 1892, *No.* 39, *p.* 519.)

In this paper the author details some very interesting experiments on the loss by hysteresis in a magnetic circuit. In making experiments on the loss when the iron was subjected to a symmetrical cycle of magnetising force, the author had found that the formula

$$W = K B^{1 \cdot 6},$$

where W is the loss per cubic centimetre, B the maximum induction density, and K a constant depending on the iron. Experiments were made to find out whether this empirical formula held for unsymmetrical cycles, which led to the result that

the loss of power by hysteresis depends on the difference of the limits of change of induction and not on their absolute value; so that the loss is the same so long as the amplitude of the induction cycle is constant; and that consequently the general form of the equation given above is

$$W = R\left(\frac{B_1 - B_2}{2}\right)^{1 \cdot 6},$$

where B_1 and B_2 are the limits of the induction cycle. Including the eddy-current loss, we have the empirical formula

$$W = R\left(\frac{B_1 - B_2}{2}\right)^{1 \cdot 6} + l\,N\left(\frac{B_1 - B_2}{2}\right)^2,$$

where N is the frequency and "l" the eddy-current coefficient. The remainder of the paper, which is too elaborate to abstract, contains very valuable curves and figures respecting the magnetic qualities of various materials. The following is perhaps the most characteristic conclusion:—For middling and high inductions, the behaviour of all magnetic substances in all important respects can be expressed by the use of three constants, α, σ, η—where

α = the coefficient of "magnetic hardness."
σ = the coefficient of "magnetic saturation."
η = the coefficient of hysteresis.

These constants may be expressed as follows:—

$B_\infty = \frac{1}{\sigma}$ = value of absolute magnetic saturation.

$F_0 = \frac{\alpha}{\sigma}$ = value of the magneto motive force which produces a $B = \frac{1}{2} B_\infty$; if for this value F_0 the straight line law magnetic resistance still holds,

$H_\infty = \eta\, B_\infty^{1 \cdot 6}$

maximum value of the hysteresis loss, which would be attained at maximum saturation.

The hysteresis loss then becomes

$$W = H_\infty \left(\frac{B_1 - B_2}{2\,B_\infty}\right)^{1 \cdot 6}.$$

The formulæ appear to hold for all magnetic substances, and were determined for all kinds of iron and steel, nickel cobalt, magnetite, &c.

MUCULESCU—MECHANICAL EQUIVALENT OF HEAT.

(*La Lumière Electrique*, *No.* 29, *Vol.* 45, *p.* 145.)

According to the *Physikilische Review*, Mr. Muculescu has obtained for the mechanical equivalent of heat the value

$$J = 426{\cdot}70 \text{ kilogrammetres},$$

in which the whole number is considered correct and the decimals as probably so. This number is given for Paris, where $g = 980{\cdot}96$. If a correction be made in Joule's observations for this value of g, the mean value becomes

$$J = 426{\cdot}5 \text{ kilogrammetres.}$$

The same article contains two interesting tables relating to various determinations of the mechanical equivalent of heat, and to methods employed by divers observers. We here give these tables:—

I.—Direct Methods.

Date.	Observers.	Methods.	Results in K.G.M.
1843	Joule (1)	Friction of water in pipes	426·6
,,	,, (1)	Heat produced by magneto-electric currents	460
,,	,, (1)	Diminution of heat in an active cell ...	442·2
1845	,, (2)	Compression of air...	443·8
,,	,, (2)	Expansion of air	437·8
,,	,, (2)	Friction of water in a calorimeter ...	488·3
1847	,, (3)	,, ,, ,, ,, ,, ...	428·9
1850	,, (3)	,, ,, ,, ,, ,, ...	423·9
,,	,, (3)	,, ,, mercury in a calorimeter...	424·7
,,	,, (4)	,, ,, iron discs ,, ,, ...	425·2
1857	Favre (5)	Diminution of heat in an active cell ...	426—464
,,	Hirn (6)	Friction of metals	371·6
1858	,, (6)	,, ,, ...	400—450
,,	Favre (7)	,, ,, in a mercury calorimeter	413·2
,,	Hirn (6)	Work on metals	425
1860-61	,, (6)	Friction of water	432
,,	,, (6)	Flow of a liquid under high pressure...	433
,,	,, (6)	Hammering of lead	425
,,	,, (6)	Friction of water between two cylinders	432
,,	,, (6)	Expansion of air	440
,,	,, (6)	Steam engines	420—432
1865	Edlund (8) ...	Expansion and contraction of metals ...	428·3—443·6
1870	Violle (9) ...	Heating of a disc between the poles of a magnet	435
1875	Puluj (10) ...	Friction of metals	425·2—426·6
1878	Joule (11) ...	Friction of water in a calorimeter ...	423·9
1879	Rowland (12)...	,, ,, ,, ,, ...	429·7—425·8
1891	D'Arsonval (13)	Heating of a cylinder in a magnetic field	421—427

(1) Joule, *Philosophical Magazine*, *t.* 23, *p.* 442.
(2) ,, ,, ,, *t.* 26, *p.* 369.
(3) ,, ,, ,, *t.* 27, *p.* 312.
(4) ,, ,, *Trans.*, *p* 61, 1850.
(5) Favre, *Comptes Rendus*, *t.* 45, *p.* 56.
(6) Hirn, *Theorie Mecanique de la Chaleur*, 3rd Edition.
(7) Favre, *Comptes Rendus*, *t.* 46, *p.* 337.
(8) Edlund, *Pogg Ann. t.* 126, *p.* 539.
(9) Violle, *Ann. Chim. Phys.*, *t.* 21 *p.* 64.
(10) Puluj, *Wien. Ber.*, *t.* 71, *p.* 667.
(11) Joule, *Phil. Trans.*, p. 365, 1878.
(12) Rowland, *Proc. Ann. Soc.*, *t.* 7, *p.* 75.
(13) D'Arsonval, *La Lumière Electrique*, *t.* 39, *p.* 534.

II.—INDIRECT METHODS.

Date.	Observers.	Methods.	Results in K.G.M.
1842	Mayer (1)	By the equation $I = p_0 v_0 a/C - c$ ...	365
1857	Quintus Icilius (2)	Heat developed in a wire of known resistance	399·7
"	W. Thomson (3)...	Electro-chemical equivalent of water 0·009376	432·1
"	Favre & Silbernann	Development of heat by action of Z_n on $C_u SO_4$	432·1
"	Bosscha (4)	Electro-motive force of Daniell cell ...	432·1
1859	Joule	Heat in a Daniell cell	419·5
"	Bosscha...	Electro-motive force of Daniell cell ...	419·5
"	Lenz-Weber... ...	Heat developed in a wire of known resistance	396·4—478·2
1867	Joule (5)	" " " "	429·5
1878	Weber	" " " "	428·15
1888	Perot (6)	By the equation $L = T/E(M' - M)\frac{dp}{dt}$	424·63
"	Dicterici (7)... ...	Joule's heat	432·5

(1) Mayer, *Lieb. Ann.*, *t.* 42, *p.* 34.
(2) Quintus Icilius, *Pogg. Ann.*, *t.* 101, *p.* 63.
(3) W Thomson, *Phil. Mag.*, *t.* 2, *p.* 1.
(4) Bosscha, *Pogg. Ann.*, *t.* 101, *p.* 517.
(5) Joule, *Rep. Comm. Electr. Standards B.A.*, *p.* 175.
(6) Perot, *Journal de Phisique*, *t.* 7, *p.* 129.
(7) Dicterici, *Wiedem. Ann.*, *t* 33, *p.* 417.

P. CURIE—MAGNETIC PROPERTIES OF BODIES AT DIFFERENT TEMPERATURES.

(*Comptes Rendus*, *Vol.* 115, *No.* 20, *p.* 805.)

To study the magnetic properties of certain bodies, Mr. P. Curie adopts a method similar in principle to that employed by Becquerel and Faraday.

An electromagnet was so arranged that the body to be experimented upon might be placed at any point in the plane of symmetry between the poles of this magnet. The force f along this plane was measured by using the torsion of a wire, the motion being in all cases very small, was found by the combined use of a microscope and micrometer. If Hy stand for the intensity of the field in the body, in a direction oy at right angles to the plane of symmetry between the poles, $M =$ mass, $I = \frac{\text{Magnetic moment}}{\text{Mass}}$, then $f = MI\frac{d\,Hy}{dn}$, Oz being the line in which the force f acts at right angles to oy.

Curves were obtained connecting values of x with Hy, $\frac{d\,Hy}{dn}$, $Hy\,\frac{d\,Hy}{dn}$.

The current in the coils of the electromagnet being gradually excited by a current of + 8 amperes to − 8 amperes through a series of identical cycles.

A preliminary determination was made of values of H and $\frac{d\,Hy}{dn}$ for various strengths of increasing and decreasing currents, then it was only necessary during magnetic observations to measure current in order to find strength of field.

All determinations of field strengths were made by using an exploring coil in conjunction with a ballistic galvanometer.

To determine the magnetic constants at different temperatures, a porcelain furnace was employed. The electromagnet being surrounded by water jackets, it was possible to study the magnetic properties of certain bodies from a temperature equal to that of the surrounding atmosphere to a temperature of 1,500 degrees.

E. SCHULZ—ADVANTAGES AND DISADVANTAGES OF 2-POLE MACHINES AS AGAINST THOSE OF MULTIPOLAR TYPE.

(*Electrotechnische Zeitschrift*, 1892, *No.* 34, *p.* 455.)

The author evolves from calculations from actual machines the general considerations which affect the question, and shows that the armature reaction in multipolar machines is a much more serious matter than in 2-polar dynamos, quoting Mordey and Andrews to confirm this view. He comes to the following conclusion: (1) The 2-polar type is the only one which can be constructed without the extravagant use of iron; (2) the constant sparkless position of the brushes is more easily attained in the 2-pole machine than in any other; (3) the 4-pole type for given speed and given weight has 36 per cent. greater output, and is therefore more suited to slow-speed machines; (4) for large machines the multipolar type surpasses the 2-polar for low tensions, as the limit caused by the small number of armature conductors is so soon reached in the latter type, and would be thus more suited for purposes of electrolysis.

LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Months of JANUARY and FEBRUARY, 1893.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

F. UPPENBORN—The Electric Central Stations of Schuckert & Co., 1, Barmen.—*E. T. Z*, No. 1, 1893, p. 1. (I.)

ANON.—The Frankfort-Lauffen Transmission.—*E. T. Z.*, No. 2, 1893, p. 18. (I.)

C. P. FELDMANN and C. D. NAGTGLAS-VERSTEEG—The Relations between Candle-Power, Pressure, and Conversion of Energy in Modern Glow Lamps.—*E. T. Z.*, No. 5, 1893, p. 60. (I.)

C. E. BROWN—Non-synchronous Alternate Current Motors.—*E. T. Z.*, No. 7, p. 81; *Lum. El*, vol. 47, No. 8, p. 371. (I.)

A BLONDEL—Running Alternators in Parallel.—*Bul. Soc. Int.*, vol 10, No. 94, p. 6; *Lum. El.*, vol. 47, No. 1, p. 34. (I.)

— DE BOVET—Magnetic Clutches.—*Bul. Soc. Int.*, vol. 10, No. 94, p. 19. (I.)

G. RICHARD—Electric Railways and Tramways.—*Lum. El.*, vol. 47, No. 1, p. 12. (I.)

F. GUILBERT—The Hutin and Leblanc Systems of Transforming Alternate into Direct Currents.—*Lum. El.*, vol. 47, No. 2, p. 51. (I.)

G. RICHARD—Recent Arc Lamps —*Lum. El.*, vol. 47, No. 4, p. 158. (I.)

ANON.—The Mülhausen Central Station.—*Lum. El*, vol. 47, No. 4, p. 179. (I.)

G. RICHARD—Incandescent Lamps.—*Lum. El.*, vol. 47, No. 5, p. 212. (I.)

ANON.—Cattori System of Electric Traction.—*Lum. El.*, vol. 47, No. 5, p. 233. (I.)

ANON.—The Dessau Central Station.—*Lum. El.*, vol. 47, No. 6, p. 266; No. 7, p. 312. (I.)

G. RICHARD—Electric Welding.—*Lum. El.*, vol. 47, No. 7, p. 302. (I.)

F. GÉRALDY—Earthing Conducting Circuits.—*Lum. El.*, vol. 47, No. 7, p. 310.

— PICOU—Electric Brake.—*Lum. El.*, vol. 47, No. 7, p. 328. (I.)

J. P. ANNEY—The Genoa Power Station.—*Lum. El.*, vol. 47, No. 8, p. 351. (I.)

HUTIN and LEBLANC—The Brown Alternate Current Motor.—*Lum. El.*, vol. 47, No. 8, p. 371.

MAGNETISM AND DYNAMO DESIGN.

A. FÖPPL—Theory of Residual Magnetism.—*W. A.*, vol. 48, No. 2, p. 252.

H. H. BROGAU—A New Magnetic Material.—*Beibl.*, vol. 17, No. 1, p. 52; *E. T. Z.*, vol. 13, 1892, p. 875.

— PISATI—A Disturbing Phenomenon in the Transmission of a Temporary Wave of Magnetism.—*Beibl.*, vol. 17, No. 1, p. 53.

R. LANG—Ohm's Law as a Fundamental Law of Magnetism.—*Beibl.*, vol. 17, No. 1, p. 54.

V. A. JULIUS and N. VAN HUFFEL—Transmission of Magnetic Impulses in a Rod. —*Beibl.*, vol. 17, No. 1, p. 54.

M. CANTONE—Appendix to Researches on Alterations of Resistance of Nickel in a Magnetic Field.—*Beibl.*, vol. 17, No. 1, p. 55.

RIMINGTON and W. SMITH—Experiments in Electric and Magnetic Fields Constant and Varying.—*Phil. Mag.*, vol. 35, No 212, p. 68. (I.)

E. EGGER—Magnetic Reactions in Dynamo Construction and Working.—*E. T. Z.*, No. 1, 1893, p. 5. (I.)

R. ARNO—A Rotating Electric Field and Rotations produced by Electrostatic Hysteresis.—*E. T. Z.*, No. 2, 1893, p. 17. (I.)

E. ARNOLD—A Problem in Alternate Current Working.—*E. T. Z.*, No. 3, 1893 p. 30.

E. ARNOLD—A Contribution to the Design of Alternate Current Motors.—*E. T. Z.*, No. 4, 1893, p. 42. (I.)

J. FISCHER-HINNEN—The Reaction of the Armature Current on the Magnetic Field.—*E. T. Z.*, No. 5, 1893, p. 53. (I.)

C. P. STEINMETZ—Influence of Freqnency on the Working of the Alternate Current Transformer.—*E. T. Z.*, No. 5, 1893, p. 57. (I.)

G. ROESSLER—Researches on the Magnetisation of Iron by Very Small and Very Large Forces.—*E. T. Z.*, No. 8, 1893, p. 97. (S.)

Th. MOUREAUX—The Value of the Magnetic Elements on Jan. 1, 1893.—*C. R.*, vol. 116, No. 2, p. 57.

P. CURIE—The Magnetic Properties of Bodies at Various Temperatures.—*C. R.*, vol. 116, No. 4, p. 136.

G. RICHARD—Details in Dynamo Construction.—*Lum. El.*, vol. 47, No. 3, p. 107. (I.) (S.)

TELEGRAPHY AND TELEPHONY.

C. GRAWINKEL—The Influence of the Middle Wire in a Three-Wire System on Telephone Conductors.—*E. T. Z.*, No. 5, 1893, p. 62. (I.)

H. WETZER—Calling the Exchange.—*Jour. Tel.*, vol. 17, No. 2, p. 25.

ANON.—Telegraphs and Telephones in Belgium, Holland, and Germany in 1891.—*Jour. Tel.*, vol. 17, No. 2, p. 29, *et seqq.*

ACCUMULATORS.

ANON.—The Boese-Lütcke Cell.—*E. T. Z.*, No. 7, 1893, p. 85. (I.)

STATIC AND ATMOSPHERIC ELECTRICITY.

A. HEYDWEILER—The Electrification of Air by the Brush and Glow Discharge.—*W. A.*, vol 48, No. 1, p. 110. (I.)

On the Transference of Electricity through Gases (4). Discharge Potential Differences.—*W. A.*, vol. 48, No. 2, p. 213.

J. Elster and H. Geitel.—Observations of Atmospheric Potential and Ultra Violet Solar Radiation.—*W. A.*, vol. 48, No. 2, p. 338. (I.)

Campbell-Swinton.—Experiments with High Frequency Electric Discharges.—*Phil. Mag.*, vol. 35, No. 213, p. 142.

A. E. Foote—Notice of Meteoric Stone seen to Fall at Bath, S. Dakota—*Phil Mag.*, vol. 35, No. 213, p. 152.

L. Palmieri—Researches in Atmospheric Electricity.—*Lum. El.*. vol. 47, No. 2, p. 69; No. 4, p. 157. (I.)

INSTRUMENTS AND MEASUREMENTS.

G. Quincke—A New Form of Magnetic and Electric Measuring Instrument.—*W. A.*, vol. 48, No. 1, p. 25. (I.)

F. Heerwogen—A Null Method of Measurement of Specific Inductive Capacity of Conducting Liquids.—*W. A.*, vol. 48, No. 1, p. 35. (I.)

H. O. G. Ellinger—The Refractive Index of Electric Waves in Alcohol.—*W. A.*, vol. 48, No. 1, p. 108.

H. du Bois and H. Rubens—Modified Astatic Galvanometer.—*W. A.*, vol. 48, No. 2, p. 236. (I.)

C. Christiansen—Apparatus for the Determination of the Mechanical Equivalent of Heat.—*W. A.*, vol. 48. No 2, p. 374. (I.)

A. Winkelmann—The Use of the Telephone in Electric Null Methods.—*W A.*, vol. 41, No. 2, p. 384.

— Mayençon—Thermogalvanoscope.—*Beibl.*, vol. 17, No. 1, p. 52; *Lum. El.*, 45, 1892, p. 627.

J. Morin—A New Form of Induction Coil.—*Beibl*, vol. 17, No. 1, p. 58; *C. R.*, Vol. 115, 1892, p. 389.

A. H. Borgesius—Bifilar-Electrometer, and Researches on Discharge Potentials in Air.—*Beibl.*, vol. 17, No. 1, p. 61.

C. E. Guilleaume—Electrical Mercury Standards.—*Beibl.*, vol. 17, No. 1, p. 63; *C. R.*, Vol. 115, 1892, p. 414.

H. Behn-Eschenburg—Researches on the Electrical Qualities of a Gutta Percha Cable.—*Beibl.*, vol. 17, No. 1, p, 69.

A. Weisshold—Apparatus for Demonstrations with Rotating Fields.—*Beibl.*, vol. 17, No. 1, p. 71.

A. Heydweiller—The Determination of High Potentials.—*E. T. Z.*, No. 3, 1893, p. 29. (I.)

C. E. Guilleaume—The Temperature Coefficient of Mercury.—*C. R.*, vol. 116. No. 2, p. 51.

— Blondel—The Measurement of Power in Polyphase Currents.—*C. R.*, vol. 116. No. 2, p. 54.

Bouquet de la Grye—An Instrument for Demonstrating Small Variations in Gravity.—*C. R.*, vol. 116, No. 8, p. 341. (I.)

J. Blondin—On Recent Researches on the Measurement of Temperatures by Electric Methods.—*Lum. El.*, vol. 47, No. 1, p. 21; No. 2, p. 75; No. 3, p. 125 (S.) (I.)

THEORY.

H. EBBERT – The Amplification of Maxwell's Theory.—*W. A.*, vol 48, No. 1, p. 1.

L. BOLTZMANN—On a Medium whose Mechanical Properties are Represented by Maxwell's Electro-magnetic Equations.—*W. A.*, vol. 48, No. 1, p. 78. (I.)

L. BOLTZMANN—On Some Questions affecting Maxwell's Theory of Electricity.—*W. A.*, vol. 48, No. 1, p. 100.

L. SILBERSTEIN—On the Movement of an Electrified Body in a Dielectric.—*W. A.*, vol. 48, No. 2, p. 262. (I.)

P. CULMANN—On the Value of Kirchhoff's Formulæ for Electro-magnetism.—*W. A.*, vol. 48, No. 2, p. 380.

V. A. JULIUS—The Conductivity of Electrolytes.—*Beibl.*, vol. 17, No. 1, p. 48.

M. COUETTE—The Constancy of the Electro-chemical Equivalent.—*Beibl.*, vol. 17, No. 1, p. 51; *J. de Phys.* (3), 1, p. 350, 1892.

F. SANFORD—A Necessary Modification of Ohm's Law.—*Phil. Mag.*, vol. 35, No. 212, p. 65. (I.)

B. GALITZINE—On Radiant Energy.—*Phil. Mag.*, vol. 35, No. 213, p. 113.

J. MACGREGOR – Contact Action and the Conservation of Energy.—*Phil. Mag.*, vol. 35, No. 213, p. 134.

J. HERZOG—Distribution of Current in Conducting Networks.—*E. T. Z.*, No. 1, 1893, p. 10. (I.)

D. CORDA—Theory of a Condenser Working on a Transformer.—*E. T. Z.*, No. 6, 1893, p. 69. (I.)

V. BJERKNES—The Dispersion of Electric Energy in a Hertz Resonator.—*E. T. Z.*, No. 6, 1893, p. 72. (I.)

E. CARVALLO—A New Law of Electro-magnetic Induction.—*Lum. El.*, vol. 47, No. 1, p. 18.

F. GUILBERT—Self-induction and Capacity.—*Lum. El.*, vol. 47, No. 3, p. 117 vol. 47, No. 5, p. 222. (S.) (I.)

P. BOUCHEROT—Condensers Acting Directly and Through Transformers.—*Lum. El.*, vol. 47, No. 4, p. 151. (I.)

F. GUILBERT—Self-induction and Capacity.—*Lum. El.*, vol. 47, No. 6, p. 261. (S.) (I.)

— CLAVENAD—On Vibratory Movements in an Isotropic Medium.—*Lum. El.*, vol. 47, No. 6, p. 272.

P. JANET – Determination of Coefficient of Self-induction by Electric Oscillations. —*Lum. El.*, vol. 47, No. 7, p. 336.

VARIOUS.

L. BOLTZMANN – On Phenomena connected with Radiant Electricity Analagous to Newton's Colour Rings.—*W. A.*, vol. 48, No. 1, p. 63.

P. DRUDE—The Calculation of Magneto-Optical Phenomena.—*W. A.*, vol. 48, No. 1, p. 122.

W. JAEGER—Note on the Purification of Mercury.—*W. A.*, vol. 48, No. 1, p. 208.

C. BARUS AND E. SCHNEIDER—The Behaviour of Colloidal Silver towards an Electric Current.—*W. A.*, vol. 48, No. 2, p. 327.

H. PEROT—On Hertz Oscillations.—*Beibl.* vol. 17, No. 1, p. 59; *C. R.*, Vol. 114, 1892, p. 165.

F. BREISIG—The Influence of Light on Electric Discharges in Various Gases.—*Beibl.*, vol. 17, No. 1, p. 60.

H. AND R. SCHRÖDER—The Connection of Electricity with Chemical Action.—*Beibl.*, vol. 17, No. 1, p. 69.

R. THRELFALL—The Electrical Properties of Pure Substances (Part I.).—*Phil. Mag.*, vol. 35, No. 212, p. 1. (S.) (I.)

H. RUBENS AND W. SNOW.—On the Refraction of Rays of Great Wave Length in Rock-Salt, Sylvite, and Fluorite.—*Phil. Mag.*, vol. 35, No. 212, p. 35. (I.)

J. LEFEVRE.—On the Attraction of Two Plates Separated by a Dielectric.—*Phil. Mag.*, vol. 35, No. 212, p. 78.

W. E. SUMPNER—The Diffusion of Light.—*Phil. Mag.*, vol. 35, No. 213, p. 81. (I.)

H. BAGARD—On Thermo-Electric Phenomena between Two Electrolytes.—*C. R.*, vol. 116, No. 1, p. 27. (I.)

— BIRKELAND—Electric Waves in Wires.—*C. R.*, vol. 116, No. 3, p. 93.

P. JANET—Hysteresis and Viscosity in Mica with Rapid Electric Oscillations.—*C. R.*, vol. 116, No. 8, p. 373. (I.)

G. RICHARD—Mechanical Applications of Electricity.—*Lum. El.*, vol. 47, No. 2, p. 61; No. 6, p. 251; No. 8, p. 362. (S.) (I.)

L. BRASSE—The Preparation of Gutta Percha.—*Lum. El.*, vol. 47, No 2, p. 101; No. 5, p. 216. (S.) (I.)

ANON.—The Vogt Dry Cell.—*Lum. El.*, vol. 47, No. 3, p. 130. (I.)

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS.—No. 105.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII. 1893. No. 105.

The Two Hundred and Forty-ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 9th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 23rd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Arthur P. Chattock. | Guy C. Fricker.

From the class of Students to that of Foreign Members—

Venesto Cirla.

From the class of Students to that of Associates—

Horace James Brydon. | William Barton Clarke.
Cecil Buchanan Clay. | A. G. Palgrave.

Mr. H. W. Handcock and Mr. R. W. Weekes were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Mr. John Aylmer, Local Hon. Sec. in Paris; Mr. Conrad Cooke, Member; and Mr. A. A. Campbell-Swinton, Member; to whom the thanks of the meeting were duly accorded.

The PRESIDENT: We will now resume the discussion on the paper read on the last occasion by Mr. Mordey. Professor Thompson, I think, wishes to supplement the remarks he made on that occasion.

Professor Thompson.

Professor SILVANUS P. THOMPSON: As I had the opportunity of saying something about Mr. Mordey's paper last week, I will be very brief. The two curves of Fig. 6 are extremely instructive as to the behaviour of alternate-current machines running as motors. What would these curves lead us to if we could run a motor without any load whatever, even without friction on the armature? In that case, of course, we should require no current to drive it round. The curve, which in these examples bends down and comes to a minimum at some point above the zero line, would in that case go down to zero. We should then have for the curve two straight lines meeting at a point. If you have the motor in that condition, running with no load on it, and running at the right speed, and if you excite its field magnets exactly up to the proper point, its E.M.F. will precisely balance at every instant the E.M.F. coming in from the outside. You will have, therefore, a wave form for the motor's E.M.F. which will be the exact counterpart of that of the impressed E.M.F., only being opposed to it always. The two curves will be absolutely symmetrical, but opposed. But if you have not excited the field at exactly the right amount, the machine running at its proper speed will not give a back E.M.F. equal to the impressed E.M.F. It is obvious there will then be some current, and that current will be more than sufficient to drive the machine if the current is absolutely in phase with the impressed E.M.F. In that case the motor will lead in phase. Again, supposing the back E.M.F. is greater than the impressed E.M.F.—that is to say, the field too much excited—you will have a current which will virtually drive

the current back from the machine to the generator—that is to say, this motor running with too highly excited a field, if it could keep in step, would be a dynamo that would send the current the other way, in which case a load would come upon it and it would be made to lag, and as a consequence the current would again get out of phase with the E.M.F. We should then have the current which runs through the machine greater than the current necessary to drive it, in which case it will settle down to a certain difference of phase; the current running up and down in the wire out of phase with the E.M.F., that is to say, as a wattless current. If now we consider the case of the motor running with a load, there will be a certain minimum current necessary to produce the requisite torque at the speed; and if the field is not exactly excited to the right amount, a larger current than necessary will flow, and that current we may resolve into two parts—the working current in phase with the E.M.F. and the wattless current at right angles to it in phase. The latter will simply run up and down the wire, demanding energy because the wire offers resistance, but nothing else. Whether the motor is under-excited or over-excited, you have excesses of current on either side of the minimum point on the curve; you have therein the measure of how much the wattless current exceeds the working current. Of course, if the motor is over-excited, the current in it will lag as compared with the E.M.F. If the motor is under-excited, the motor will have to lead in order that the thing may work; so that you have in the amount by which the current grows up on either side from the minimum an exact measure of the amount by which the phase is altered, from complete opposition, for leading or lagging, according to whether the excitation is below or above the minimum. If the magnetism of the motor field were at every moment proportional to the exciting number of amperes, then I presume those two limbs of the curve would be symmetrical; at any rate, the two asymptotes would slope equally. But we know the magnetism is not so proportional. The only reason why the curve is nearly symmetrical is because in these machines the field magnet is not excited to anywhere near what one calls the saturation point of

Professor Thompson.

Professor Thompson.

excitation. Therefore the magnetism on either side of that minimum point is not very far from being proportional to the exciting current.

We have to thank Mr. Mordey for a most excellent phrase for expressing the general mode of testing machines which was introduced by Dr. Hopkinson, and worked out so thoroughly in this paper; that is, the method of "circulating" the power by taking the power out of the machine as a motor, and putting it back into it to drive it as a generator. That is a very happy term.

I happen to have been reading to-day the proceedings of one of the American societies, the National Electric Light Association, for the year 1892, in a volume that came to me a few days ago, and I was very much amused to see a communication to that body read by an engineer, Mr. Leslie, who was running a certain Manhattan station with alternating-current machines. He was describing the advantage of a new system of switch-boards, which he said was a beautiful switch-board, made of slate marbleised in imitation of Tennessee marble—a real ornament to any station. It was contrived on the following principle:—There were 17 different circuits going out from that station to the lamps, and 19 different alternating dynamos, and the arrangement was such that "it is absolutely impossible to get two alternators in parallel "through any mistake of the operator." There could hardly be a more striking commentary upon the difference between one country and another than the paper discussed here to-night and that read in America, showing how the practice in the two countries has gone absolutely in two different directions.

Dr. Fleming.

Dr. J. A. FLEMING: The remarks I should like to make on this interesting paper will have reference chiefly to the instrumental means for measuring the electrical quantities which it is necessary to measure in Mr. Mordey's electrical method of determining the efficiency of alternators. I have placed on the table before me a new standard wattmeter which has been constructed for me by Mr. Shields, which is intended for the purpose of measuring alternating-current power over very large ranges and with great accuracy, and I think we have produced an instrument which will

be of great service in measuring not only small powers with considerable accuracy, but also very large ones. The principle which is employed is one which I described very fully in another place quite recently; it is simply a wattmeter in which the shunt coil is excited by means of a transformer. If you will look at the instrument afterwards you will see that inside the wooden box which contains the whole thing there are two fixed coils. These two fixed coils are the series coils of the instrument, and can be joined either in series or in parallel with one another; they are wound with No. 18 copper wire, and they will carry up to, say, 10 amperes. The movable coil is a rectangular coil of four turns of No. 16 wire, which will carry up to about 20 amperes, and that coil is suspended top and bottom by a phosphor-bronze wire. It is intended to associate with the wattmeter a transformer for stepping down from 2,000 or 2,400 volts to about 20 volts, and that transformer will have its low-pressure circuit joined in series with the movable coil, and will have a series of small resistances of platinoid, which can be put three or four or more in parallel as required so as to regulate the current that has passed through the movable coil, regulating it from, say, 1 to 20 amperes. The transformer will have its primary joined across the high-tension mains of the alternator or the transformer which is being tested. Additional regulation is provided in the way in which the fixed coils are adjusted. In ordinary wattmeters the fixed or series coils are firmly fixed, but in this instrument they are carried by two pieces of wood and are capable of being pushed together or drawn apart by screws so as to bring them nearer or move them farther from the movable coil. The movable coil has the current got into it in the following manner: instead of fixing the mercury cups, they are carried on the coil itself. Passing through the movable coil, and at right angles to it, are two wires which bring the current into the mercury cups, and they have little copper points which dip down into the mercury cups, and the current is brought in at both ends of these wires, the object of that being to produce perfect symmetry in getting the current into and out of the movable coil. We were led to adopt this arrangement because in some other experiments we passed a current of 20 amperes through the

Dr. Fleming.

Dr. Fleming movable coil, and when no current was going through the fixed coils we still found that we got a large deflection of the instrument. We ultimately found that this was due to want of symmetry in the current brought into the movable coil, and was due to the reaction between the current in the movable coil and that in the leads bringing in the current. We got rid of that, but even then there was a small outstanding deflection, and the reason for that we have not been able quite to trace. It appears as if a coil carrying an alternating current will sometimes displace itself under the influence of the earth's magnetism alone. That may be due, perhaps, to the fact that in some alternating currents the current is more one way than the other; there is not perfect symmetry in the current. It is quite possible for this to happen, and if one could exaggerate that a good deal one might obtain curious effects. I know of alternating currents which can decompose solution of sulphate of copper and deposit copper on one terminal. It acts in that respect entirely like a continuous current. By the adoption of perfect symmetry you get rid of the troublesome deflection due to reaction of the leads. The great adjustability of the instrument rests in the fact that we can vary the current of the movable current to a great extent—from 20 amperes down to a small fraction of an ampere—and therefore, if we have currents in the fixed coils up to, say, 10 amperes, we have an instrument capable of measuring from 200,000 watts downwards to about 10 watts, and we shall expect to find that we can get for those great ranges an accuracy of one-quarter per cent. The instrument before you with 100 watts would require a whole turn of the torsion-head to bring the indicating needle back to zero, and as the scale is divided into quarter degrees, we have a considerable range of reading in the instrument, and we shall be able to obtain a very high degree of accuracy with it. The reason for having the fixed coils displacable is in order to obtain an instrument in which you do not require to multiply by a constant at every observation. We can take a circuit absorbing, say, 200 watts, put the wattmeter on to it, and adjust the distance of the fixed coils in such a way that it requires a torsion of 200 degrees to bring the indicating needle back to zero. In this way

you get over the necessity of troublesome calculations at each observation, and make the reading direct. There is a very ingenious method of attaching the wire which carries the movable coil to the spindle, due to Mr. Shields. It is a matter of some difficulty to attach a wire to a spindle perfectly centrally, and, at the same time, by a method which admits of great ease of adjustment. The spindle is cut away to a square notch, and in that spindle a hole is cut exactly in the axis of the spindle and another one at the side of the notch, and the wire to be attached centrally to the spindle is brought up through the bottom hole and out at the side one, and a little pin is put in the side hole to tighten the wire. The adjustment is made at top and bottom, so that the line of prolongation of the axis of the spindle is exactly the line of the suspended wire. The current is got in and out of the movable coils by the mercury cups, so that there is no current through the fine-wire suspension. I think that will be found to be a useful form of standard wattmeter in carrying out tests of the efficiency of alternators in which all the measurements are made electrically, because, by that method, you only need use one of these instruments to make the two measurements one after the other, and get the efficiency which you require to know entirely by these two measurements with the one instrument alone. Dr. Fleming.

Dr. JOHN HOPKINSON: The general method of testing the electrical appliances of which that which Mr. Mordey has so well described in his paper before us, in a particular case, has really two advantages. The first is that you are able to test your appliances with the expenditure of a very small power, namely, the power which is wasted in those instruments; the other is that you can carry out your test with a very much greater degree of accuracy. Suppose, for example, in the combination of the two appliances there is a loss of 10 per cent., and you can make your measurements with an error of only 1 per cent., then you can make your test of efficiency to an accuracy of 1-10th per cent., or thereabout. That second advantage I am not sure whether Mr. Mordey puts in sufficient prominence. The advantage, however, exists, whether the dynamometer be used, or whether you use an Dr. John Hopkinson

Dr. John Hopkinson.

arrangement proposed afterwards by Lord Rayleigh, in which all the measurements are made electrically. About a year ago I tried some experiments with transformers by this method. The method I then adopted was substantially the same; that is to say, we measured the power transmitted from one instrument to another, and we measured directly the loss in those two instruments. As applied, then, that method had only the second of those two advantages; it had not the first of economising the power required to drive the combination. Immediately afterwards, almost—I daresay the experiments were done before my own were done, but they did not happen to be published till then—Professor Ayrton and Dr. Sumpner published a description of tests of transformers which realised both those advantages. They were such that economy of power was secured as well as greater accuracy of testing. My method, however, was designed to give other information than the mere testing of efficiency. As the transformers were sent to me, undoubtedly the test of efficiency was that to which I was asked to address myself, but having them in my hands I tried to get out of them all the information that was obtainable, and I think that we succeeded.

Mr. Mordey refers to the possibility of testing a single continuous-current machine in which there are two equal circuits with separate commutators, by connecting those two commutators together in such wise that the power developed electrically in one circuit will be used in the other to generate mechanical power again. A method applied in that way is, of course, as I think Professor Ayrton pointed out last time, open to the objection that it does not instantly give you a complete test of the mechanical properties of the machine; you do not get the mechanical torsion applied to the shaft, and generally the mechanical properties of the machine are not tested thereby. It has also another objection, and that is this—that, if you have two coils connected in that way, you will have no distortion of the magnetic field, for the current in one section of the armature will be counterbalanced by the current in the other, and the sparking point of the transformer will be the same as if there were no current at all passing

in that machine. In the generality of continuous-current machines this is no great inconvenience, and probably does not introduce any serious error into the accuracy of the efficiency tests; but one can well imagine machines in which there would be a greater efficiency shown when tested in this way than if tested with two separate machines coupled together mechanically and coupled together electrically. Take, for example, the case of a Pacinotti ring, or let us say of the ordinary Brush dynamo. In the case of the Brush dynamo used as a generator, it is well known that the pole-pieces heat, and that they heat particularly in that part where the armature is pulled away from the pole-pieces; if, on the other hand, the machine is used as a motor, they heat at that part of the pole-pieces to which the armature approaches. Now, if you couple two machines together and test them by this method, you will have this heating developed in the machine; if, then, you arrange a machine with two commutators upon it in such wise that the current in one section of the armature opposes that of the other, you will have this heating considerably diminished; and I think it probable that you would in the latter case get a higher efficiency than in the first. Mr. Mordey's method of testing alternators in the single machine is open to a certain extent to the same objection, because you do not get that steady and regular distortion of the field that you would get if you had the armature acting simply as a generator or as a motor. If a machine be acting as a generator, you will have the intensest field in that part of the pole-piece from which the armature is receding; if the machine be acting as a motor, you will have the intensest field in that part of the field to which the armature is approaching; and it is evident if the armature is acting both as a motor and as a generator, you will have alternately the distortion of the field on one side of the pole-piece and on the other. Dr. John Hopkinson.

Mr. G. KAPP: It may be interesting to the meeting if I mention a practical example where the behaviour of alternators when used as motors has been verified in accordance with Fig. 6. I refer to the power transmission by alternating current put up two years ago in Cassel, and it may be necessary for me to say a few Mr. Kapp.

Mr. Kapp words about the general method in order to point out the analogy with Mr. Mordey's diagram. Fig. 1 is a diagram which represents

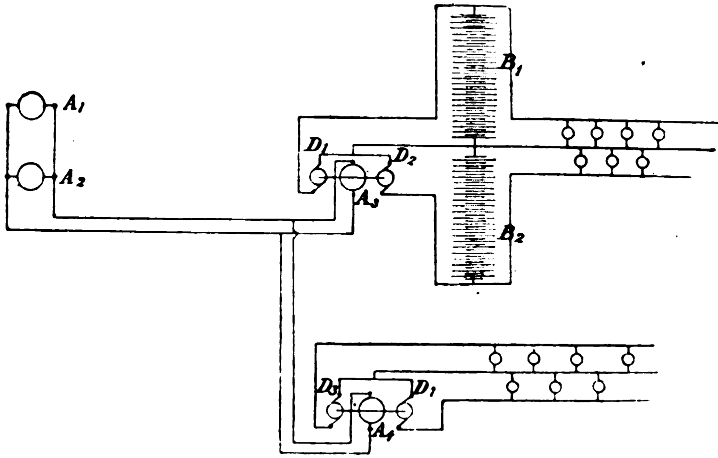


FIG. 1.

both the generator station and the motor stations. Imagine A_1 and A_2 to be two alternators coupled in parallel at the generating stations, driven by water power. The current is sent to Cassel through a concentric cable several miles long, and then splits up into two branches leading to two sub-stations in different parts of the town. The current supplied to consumers is a low-tension continuous current. The object of having two sub-stations, of course, is to save copper in the mains. At one of these two sub-stations there is a storage battery, and both sub-stations are connected to the mains on the three-wire system. Each sub-station is equipped with two continuous-current generators driven by an alternator coupled rigidly between them. When this plant was started about two years ago the men in charge made the discovery that they could vary the alternating or power current for a given load by adjusting the exciting current through the fields of the alternators. When I first heard of this effect I had not considered the question, but it led me to investigate this problem, and I worked out for a smaller machine of the same type what would, if worked as a motor, be the armature current for any given load with various excitations. I worked out from its characteristic curve and from its self-induction what would be the

armature current in order that the machine might do a certain amount of mechanical work. The result of this investigation is plotted in Fig. 2, and relates to a 10-kilowatt machine wound for **Mr. Kapp.**

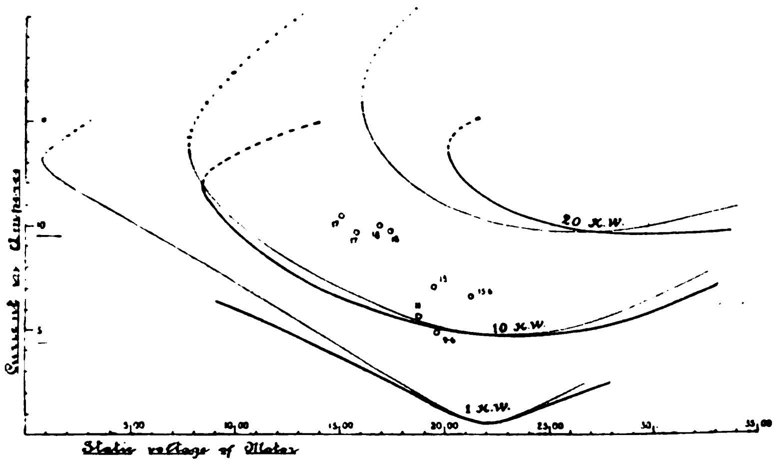


FIG. 2.

2,000 volts. I found that if the excitation was such that the machine would give 2,000 volts when working as a generator it would give the 10 kilowatts with the smallest possible armature current if working as a motor. If you excited the machine to a higher degree, so that it would as a generator give 3,000 volts, the armature current required to do the same 10 kilowatts as a motor would be greater. It would also be greater if you were to excite the machine below 2,000 volts, though it would still continue to give out the power. There is an interesting point in connection with this curve. You will see that it rounds off on the left. Now what does that mean? It means if you reduce your exciting power below a certain point—in this case an equivalent exciting power which would give 900 or 800 volts—the armature current would become very large, about 2½ times what it is when the machine is working in its proper condition. But if you reduce the exciting current below this point you would miss the curve altogether; that is to say, the machine would fall out of step and cease to give power. The diagram also shows that with the proper excitations for full power the machine may safely be

Mr. Kapp. loaded up to twice or even three times its normal power. This is confirmed by practical experience. We know that alternators will support from two to three times their normal loads before being thrown out of step. The thin-line curves result when you neglect, and the thick-line curves when you do take armature reaction into account. On the side of high excitation you see that the armature reaction always tends to increase the exciting current required; on the side of low excitation and at moderate loads the armature reaction tends to decrease the excitation, for the simple reason that we have a leading armature current adding to the excitations of the field magnets and the field current proper need not be so great. I have drawn these curves for three different powers—20 kilowatts, 10 kilowatts, and one kilowatt. To verify this investigation I tested the machine by loading it as nearly as I could to those powers. The results are plotted, and you will see that the curves are of the general character and agree with those determined theoretically. The curves shown by Mr. Mordey are for light running; my curves are for running loaded, and, therefore, the general character is different.

Mr. Harrison Mr. H. E. Harrison: Mr. Mordey acknowledges his indebtedness to Dr. Hopkinson's earlier papers on this subject, and states that his experimental results have there been anticipated. I am afraid that most practical engineers regard these forecasts rather in the light of that statue of great price which is said to lie in the proverbial block of marble, and I doubt if many engineers have ever chipped off the mathematics in which these great truths lie hidden. But Mr. Mordey has referred us to these papers, and we must look there for the explanation of his results. Dr. Hopkinson's papers first of all show two dynamos coupled up as Mr. Mordey couples up his, and then follow curves to illustrate what takes place. Now, if these curves are read side by side with the mathematics which follow, the whole matter is explained simply enough. But, unfortunately, there are those who read the curves without the text, and get hopelessly misled in consequence. First of all are shown the E.M.F.'s of each of the alternators, then the resultant E.M.F. which is got by simply adding the two together, and finally the current is drawn out of

phase with the resultant E.M.F. Some people believe that this is the case—that the current has its maximum value when the E.M.F. is not at its maximum, and, stranger still, a current of considerable magnitude when the E.M.F. is zero. Those who do not believe in miracles, even when said to be performed by dynamo machines, think there is something behind all this; there is. There is an E.M.F. which is not shown by the curves. They do not, in fact, tell the whole tale. I think that a great deal of the misapprehension that exists about the working of alternators arises from the strange way in which it is customary to speak of their E.M.F. To illustrate what I mean by a simple analogy: if you have three Daniell cells in series, you have a battery which has an E.M.F. of three volts; if you reverse one of these cells, it neutralises the E.M.F. of a second cell, and you have a battery E.M.F. of one volt. Apply Ohm's law to the circuit, and it works; but if it is maintained that the E.M.F. of such a battery is still three volts, there would be an apparent contradiction of Ohm's law such as is frequently supposed to be the case with alternating currents. Mr. Harrison

Mr. Mordey is guilty of a similar fallacy in his paper. He says Dr. Hopkinson predicted years ago that an alternator would run as a motor even when the motor had a higher E.M.F. than the generator. This statement, I think, has to be very much qualified. As Dr. Thompson has already pointed out, an alternator running at a certain speed, with a certain exciting current, has a certain E.M.F., but when the current passes through the armature the field is interfered with, the armature does not cut the lines that it would have cut, or did cut, on open circuit, and the actual E.M.F. set up may be reduced to any extent. Indeed, it would be possible to send a current through the armature which would entirely neutralise the field at every point, so that no E.M.F. would be set up. This statement, then, that a 100-volt alternator can drive a 200-volter as a motor is true only with the qualification that when it is doing so the 200-volter is not giving 200 volts. I have long suspected that there is a great deal more in the reaction which goes on between the armature of an alternator and its field than is generally supposed. At

Mr. Harrison Faraday House we have an old Siemens alternator; all technical institutes have one, because they are so instructive—some people say because they can be bought so cheaply. I have recently been trying some experiments with this alternator, but as I shall like when I do publish my results to publish them in their complete form, I do not want to say anything about them to-night, but will merely mention a single experiment. I excited the field of this alternator from a secondary battery, which, of course, normally gives a perfectly steady current. The armature was then run up to the proper speed by the gas engine, synchronised with the current in the London Company's mains, made to run as a motor and the belt thrown off. The alternating current in the armature begins to throb or pulsate and increase, and what has not been referred to by any speaker, and I do not think is generally known, is this—that the current in the fields pulsates too. The needle of the ammeter in the field circuit will pulsate in the most marked manner, and as this pulsation increases, the current in the armature increases at the same time. Under proper conditions these effects can be produced in a very marked degree.

I am of opinion that this hunting of the current is an exceedingly important factor in the running of central stations where alternators are connected abreast or used for motors. There was one point which was referred to last time by both Dr. Thompson and Mr. Mordey, as to the waste of power that takes place when two halves of the armature of an alternator are connected abreast. In this particular machine I have connected them abreast when giving 70 volts, and I find that the current which flows between them is only half an ampere, so that the amount of power which is wasted can be neglected.

This, however, is not by any means the case with the currents which are set up in the armature otherwise. I have not made any actual experiments as to power used, but if the gas engine is running the alternator empty, and the exciting current is then switched on from the cells, the armature being open circuited, there is a very marked decrease of speed, showing clearly that the amount of power so wasted is, at all events, not a quantity which can be neglected by the gas engine.

Mr. J. SWINBURNE [*communicated*]: When a paper from Mr. Mordey's pen is announced we always expect something new and something good, followed by a lively discussion. Before adopting a substitute for a full-load test of a dynamo we must carefully consider what parts of a full-load test are important, and how far they exist in the substitute. A full-load test does four things. It tests the armature for mechanical strength, the bearings for full-load behaviour, the armature coils for heating, and the field for heating under full excitation. If an efficiency test is also needed, the test gives it, including losses in bearings, with belt or ropes on, losses in the armature due to full-load current, and losses in the field under full excitation. Let us see how far Mr. Mordey's method will do as a substitute. In such a machine as his own it tests the armature for mechanical strength, practically, for the portions that are not subjected to the full forces are external and easily made strong. In a machine which has a revolving armature, however, this is no longer the case. There is no test applied to the armature hub, or shaft. Even in Mr. Mordey's form the shaft is not tested. The bearings, again, are not tested, for they are running light. The armature coils are practically tested for heating, but not completely so, as any Foucault currents in them would not be so serious. Foucault currents are very much increased by the modification of the field due to armature reactions, and this modification is lessened because the armature coils act in opposition. The field coils, again, are not tested under full excitation unless they are excited so as to give full pressure at full load. The pressure necessary for this, by the way, cannot, I think, be found by loading up one coil only. The armature reaction plays an important part in the full-load drop, and a single coil cannot act on the field in half a period. I cannot accept either of Mr. Mordey's experiments as to his single coil, as they do not agree with other experiments made on alternators. Turning now to the efficiency, the bearing losses are not given as the belt is not on. Mr. Mordey seems to think an efficiency test should be taken with the belt off. I would submit that is absurd. The loss due to the belt occurs in practical use, and must therefore be

Mr. Swinburne.

Mr. Swinburne. included in the machine losses. If not, where is it to come? The engineer may equally justly say his engine should be run without a belt because the belt is not part of the engine. Of course, the efficiency comes out higher without the belt, but the too common habit of making out a higher paper efficiency is to be deprecated, as it is really a kind of misrepresentation. Why do people, for instance, talk of the "electrical efficiency" of a machine? The purchaser wants to know the losses in the whole machine, not the losses in one portion of it. Next, the losses in the armature are not equal to the full-load losses unless the field is in the same condition as at full load. I mention these points, not with the view of raising small objections to a most valuable system of testing, but to guard against the thoughtless inaccuracy of assuming that because a machine at full load has its full armature current, a test which gives its full armature is necessarily equivalent to a full load test. These points being remembered, Mr. Mordey's system provides the electrical engineer with a most valuable means of approximately testing large dynamos.

Mr. Mordey's system is, in short, running part of the armature of a machine against the rest. This admits of very many modifications, but in patent language it is the essence of his invention, and once it becomes known various detailed developments of the principle will readily occur to those who think about the matter. Professor Ayrton, who had the good fortune to speak first, mentioned the application to multipolar direct-current machines. I had also considered the same development of Mr. Mordey's method. Take the case of a four-pole machine whose armature is parallel wound, but not connected internally. The opposite brushes can be connected through an ampere-meter, one pair of fields being slightly weakened. If the conductors are cross-connected, the brushes must be taken off and two slip-rings put on over the commutator, and the current measured with an alternating ampere-meter. If the armature is series wound, one pair of fields must be reversed. The machine is then short-circuited through an ampere-meter and the fields thrown out of balance. This is not an easy test with all dynamos. I do not know whether Professor Ayrton has gone into the question of

yoke saturation. The yokes of a well-designed multipolar will only carry half the total field of one pole easily, and they will not easily take double their normal field through them. This applies also to alternators with rotating armatures and fixed multiple fields. In a properly designed alternator you would have to alter the armature connections, not the field connections. There is a method of testing which is often useful and is not as well known as it ought to be, viz., running at full current but low speeds. It satisfies many of the conditions of full-load testing. Before leaving the subject of back to back testing, a suggestion may be made as to further developments. Why not apply the principle to steam engines? Next time a twin-engine is built for an Atlantic liner let us hope that the engines will be coupled in the erecting shop with their steam pipes coupled together, and their exhausts, the whole being run at full load, without condensation by a donkey boiler. Suppose, however, instead of showing engine builders how to do their work, we take a humble attitude and learn how to build dynamos so that we can confidently send them out without even trying them round. Mr. Swinburne.

The portion of Mr. Mordey's paper referring to the power-factor of alternators can scarcely be said to add to our knowledge of the subject, as these properties have been well known for some four years. The excitation can always be adjusted to give the smallest armature current, but the excitation which gives the smallest armature current in a motor at no load does not necessarily give it also at full load. Sometimes at full load more excitation is necessary, sometimes less; and sometimes, if the armature reaction exactly compensates for the loss by armature resistance, the best excitation remains the same. It has also been pointed out years ago that it is often best to under-excite a little so as to give the motor more "backbone," so that it will take larger loads without pulling up. I am afraid I frequently dissent from Mr. Mordey, but as to parallel running of alternators I must agree with him generally, more especially as the whole of his six recommendations coincide exactly with my own conclusions as given in *Industries* some years ago. The articles referred to

Mr. Swinburne. dealt at length with the question of coupling and controlling direct and alternating dynamos in stations. I cannot, however, agree with Mr. Mordey as to the details of parallel running. To secure parallel running you must not consider the dynamos only, but the whole station. A factor commonly neglected, and not referred to by Mr. Mordey, is the engine governor. To run a number of alternators in parallel off separate engines, each with its own independent speed governor, is as foolish as to run independently governed engines on to one shaft. The relation of steam admission to speed cannot be the same in a number of governors, and even distribution of power must occur. This cannot be corrected by altering the dynamo excitation: that alters the armature current, but not the power in this case, as it does not affect the speed.

Then there is an error in Mr. Mordey's example. This second alternator appears to give no current at no load. It must, therefore, be excited to give the station pressure, say, 2,000 volts at no load. To work in parallel on the mains properly it must be excited to give 2,000 volts at full load, and therefore something like 2,100 volts at no load. Either the machine is to be thrown into circuit with too weak a field, or the no-load current must be large enough to reduce the pressure to 2,000 volts by armature reaction. The same error comes in when Mr. Mordey cuts off his steam in the example. *Addendum.* —The error in Mr. Mordey's directions may be more easily seen by taking an example. Suppose a station has a single dynamo—No. 1—at work during the day, giving 2,000 volts on its full load, and therefore excited so as to give, say, 2,100 on no load—that is to say, fully excited. As the station load increases, more dynamos are to be put in parallel, but they are to be under-excited, and to give 2,000 volts on no load, and they would thus give 1,900 or so on full load if independent. At the heavy part of the evening you thus have, say, ten dymamos running in parallel, giving 2,000 volts and full current, of which only one is fully excited. As the demand falls off imagine No. 1 taken out of work instead of No. 10, and you have, according to Mr. Mordey, nine dynamos giving 2,000 volts each at full

load, none of which would give more than 1,900 if it worked on an independent circuit. Mr. Swinburne.

I have so frequently written on this subject that I will not weary you with it. I will only say shortly that when dynamos are in parallel the excitation controls the idle component of the armature current, and the steam admission controls the active component — that is, the output. Every station dynamo should therefore have a wattmeter to show whether the steam supply is right, and whether the machine is doing its full work, and an idle-current indicator to show whether it is over- or under-excited. At present people do not study the question as a whole. They look at a little bit of the problem—the dynamo, for example, or its armature—and they neglect the engine, the exciting arrangements, the governors, the throbbing of a slow-running engine, and the steam-pipe connections. They then couple two alternators in parallel. If they burn up, they say alternators don't run in parallel; if they do not burn up, they write a testimonial for the maker of the alternator.

As to the excitation; it is to be hoped makers will give up the absurd practice of making a lot of little exciters. It would be best to run the fields in series, not parallel. This avoids the troubles occasioned by variable resistance of fields due to temperature changes. A constant-current dynamo run by an engine with a safety governor only is more economical. During the day it runs very slowly with economical full load cut off, and does not wear out. As I have also advocated this elsewhere, I will say nothing further about it now.

If these remarks appear to be unappreciative of the great value of Mr. Mordey's paper, I trust that it will be understood that if one has studied a subject fully from his own points of view, it is unlikely he will agree completely with another engineer who has approached the same subject from what is a different and, no doubt, according to Mr. Mordey's numerous disciples, a better way. Let me heartily congratulate Mr. Mordey on another valuable paper.

Mr. R. E. Crompton: Mr. Swinburne has to some extent anticipated what I was about to say on the influence that the Mr. Crompton.

Mr. Crompton. steam engine governors have on the parallel working of alternators. Mr. Mordey at the latter end of his paper sets forth the conditions upon which we can make alternators work successfully in parallel. I agree with Mr. Swinburne that the difficulty will be found in the engine governors. Mr. Mordey hints that one way of getting over the difficulty is to supply the whole of the engines from a common steam pipe and to have one governor common to the whole. This is a proposal which I frequently discussed with the late Mr. Willans when discussing the question of the electric governing of modern stations which have steam dynamos in use coupled in parallel. Although the idea was never carried out, it appeared evident to Mr. Willans and myself that we ought to have one large electric governor on the main steam pipe which could be adjusted to meet the rise of pressure required at certain parts of the load, each of the separate sets being provided with a runaway governor which would only come into use to prevent accidents. One of the main causes why alternators have not up to the present been worked to any extent in parallel has been, not that the alternators will not keep in step, *but that each set will not take its fair share of the load*, and this is due to the difference in the sensibility of the governors employed. Even if we do as Mr. Mordey proposes and employ one main governor, there will always remain the difficulty that engines differ somewhat in their internal friction: one engine gets better lubricated than another one, and consequently the load thrown on that engine is increased, and in order that it and its dynamo may maintain the same speed, it ought to get a slightly increased supply of steam; if it does not do so, it will lag behind and will not take its fair share of the load. The problem has not yet been satisfactorily solved, and I particularly call the attention of those here who as mechanical engineers are interested in engine governing to the need there is for a speedy solution. As to the measuring of the efficiency of machines by the "circulating "method," there is no doubt that the side pull of the belts is of importance in small machines, though not so great in larger ones, so that when efficiencies are tested with this method they give efficiencies higher than those actually obtained in practice. This

is shown very plainly in the very high efficiencies that makers of dynamos have been able to show when their dynamos have been direct driven. In this case there has been no side pull and the efficiencies of 94 and 95 per cent. have been frequently obtained. Mr. Crompton.

I should like to ask Mr. Mordey why he has not availed himself of one of the means of testing the efficiency of dynamos that we have all availed ourselves of in the past, namely, of attaching an alternator to a Willans engine and then submitting it to the very excellent series of tests which are daily carried on at Messrs. Willans's works. In these tests he would obtain the double check of measuring the total horse-power from the indicator diagrams and from the total quantity of water used per horse-power per hour. This double check has been found so effective that it is a matter of common knowledge that the tests taken at wide intervals of time have been found to compare very accurately with one another, and I think that a great many of the best makers are quite satisfied that their accuracy exceeds that obtained by any other method at present known of. It will be interesting to see how far tests thus obtained will corroborate those obtained by Mr. Mordey by the methods laid down in his paper.

Dr. W. E. SUMPNER: There are one or two points in connection with Mr. Mordey's method which deserve a little attention. In the first place, Mr. Mordey describes it as being an application of Dr. Hopkinson's method to a single machine. That is true to a certain extent, but it is more true to say that it is an application of Dr. Hopkinson's method, not to one alternator split into two parts, one of which is used as a motor and the other as a dynamo, but rather to a combination of motors and dynamos. Every alternator really consists of a number of small machines in series, and what Mr. Mordey has done is to connect some up as motors and some as dynamos, and to use the Hopkinson method in connection with the combination. For that reason I think the objection that Dr. Hopkinson has mentioned does not apply to Mr. Mordey's test, because the motors are separated from the dynamos to the same extent as in Dr. Hopkinson's original test with dynamo and motor. Another advantage of this method is that it can be applied in many different ways. There is, first Dr. Sumpner.

Dr. Sumpner.

of all, what may be called the Hopkinson method, in which power is supplied mechanically, with two or three modifications which Mr. Mordey has pointed out; then there is what may be called the Rayleigh modification, all power being applied electrically to the armature. The advantage of these variations is that you can test the different losses separately. One of the great merits of Dr. Hopkinson's method, one which he has not mentioned to-night, is that you can separate the losses completely. You can not only measure the sum of the losses accurately, and not only do this with a very small amount of power, but you can very easily separate the different losses, and, as the method can be applied in many different ways, you can do this with more ease and accuracy. If you supply the power mechanically, you of course either use a belt or a direct method of driving. If you use a belt, you have a side pull on the bearings; while, if you supply all the power electrically to the armature, you do not have this side pull, and therefore you can get what the losses are independent of this side pull. As already mentioned, this method does not test the shaft, nor does it always test the extra friction due to the side pull of the driving belt, but it tests everything else. Mr. Miller mentioned last time that some tests were made at the Central Institution on an alternator in which the field was fixed and the armature moving. The field magnet in that case was split into two equal portions, the currents in the two portions being made unequal in order to make the dynamo portion stronger than the motor portion. The machine was a small one, but I have lately had the opportunity, through the kindness of Mr. Esson, of testing a 100-H.P. Gulcher alternator by the same method. The machine was one having a number of radial poles, and a drum armature with the coils wound flat on its surface somewhat as in the Westinghouse alternator. It was essentially the same as the alternator described in Mr. Esson's paper on the design of multipolar machines read before this Institution some year and a half or two years ago. I tested this machine in two different ways. The field magnet in one case was split into two equal parts; there were 20 pole-pieces, and ten of the pole-pieces were excited with a current of 18 amperes, the other ten coils were

excited with a current of 8 amperes, and the armature was short-circuited. The different losses were measured successively. First, the losses in the driving motor, when run at proper speed, were measured before the belt was put on. Other tests were made when the armature was open-circuited, both before and after the field magnets were excited. Finally, the armature was short-circuited, and another test made. In this way the different losses were separately determined. The same test was applied by splitting the field into two unequal portions, 12 magnet coils being in one part, and 8 in the other. The current in the two parts was the same. That corresponds with Mr. Mordey's method shown in Fig. 1. Instead of the armature being divided into two unequal parts of 8 and 12 coils respectively, the field magnets were divided into two unequal parts of 8 and 12. In each method of test the results obtained were satisfactory, considering the hastiness of the measurements, and the efficiency came out about 90 per cent. Those two methods are methods in which the driving power was supplied mechanically. There are other modifications in which power can be supplied electrically by passing current into the armature. They are all really the same method, and practically the method which Mr. Mordey has mentioned applied to a machine with fixed field magnets instead of to a machine with fixed armature coils. Dr. Sumpner.

The curve which Mr. Mordey shows in Fig. 6 is a very interesting one indeed, and I think the only way, or the best way at all events, in which the question of parallel running can be approached experimentally, is by finding out curves of this kind, since the study of them is more likely to show clearly what is happening electrically in a machine than any amount of mathematical reasoning on armature reactions.

Mr. FRANK BAILEY: I should like to have been able to speak from actual trial of this method of testing alternators, but I am sorry to say that I have not been able to make any practical test of it. All the alternators I have to do with are coupled up permanently in such a manner that it is difficult to disconnect them to make tests of this kind. The only point in Mr. Mordey's paper on which I would offer some remarks is that in which he Mr. Bailey.

Mr. Bailey. mentions the difficulty of getting all the magnet poles and armature coils to give uniform results, so that when coupled in parallel an equal output is obtained from each coil. It is a good many years now since the late Mr. Gordon designed some alternators for the lighting of the Great Western Railway at Paddington. These were low-pressure machines with a parallel arrangement of armature coils. I was working with him at the time, and one of our great difficulties was to ensure that the magnet poles were giving exactly the same effect. About that time, in order to test their working, we had to devise various means of first of all finding out if there were any inequality in the machine and of localising it, to see whether it was in the field-magnet coils or in the armature coils. It may be interesting to describe one method which I adopted at that time. It will make it simpler if I put it in the form of a rough diagram. This represents the poles of the field magnets; in this case they are revolving, and, for the purpose of illustration, we can place the armature coils on the exterior. We want to find first of all if there is any inequality in the magnet poles or in the armature coils. Then, to test for any inequality in the machine, we can commence by disconnecting one armature coil and observing a voltmeter placed across the remainder of the coils. Then if we have, say, twelve pole-faces, it is clear that only eleven of them are usefully employed, and when a defective pole-face passes the idle coil, the remaining coils give evidence of its absence. If we wish to test the armature coil, it is equally simple to do it on the same lines, because we can, by intentionally weakening one or more of the field-magnet coils, practically make them fault-finders. By doing that we reveal the weak point in the armature. In fact, one black sheep is used to find another, and the respective position of one being known, the other is localised.

Mr. Weekes. Mr. R. W. Weekes: I should like to say a few words about armature reaction in alternators. When working at the Central Institution in 1891 with Messrs. Thornton and Kolkhorst, we experimented on a Ferranti alternator to find out whether the self-induction of the armature altered with the load, and also whether all the armature reaction could be explained as a self-induction effect.

We first measured the self-induction of the armature when stationary with the field fully excited, and obtained the mean value for different angular positions. Mr. Weekes

Next the alternator was run up to a definite speed, and made to deliver a constant current to three totally different circuits successively, the first being a condenser, the second an inductive solenoid, and the third a non-inductive resistance.

The terminal volts were measured in each case. We thus obtained data as to the action of the machine when the current in the armature had a load or lag or was in phase with the armature E.M.F. We were thus able in each case to calculate the impressed E.M.F., and it will be seen that this is constant within experimental errors.

We made one trial with the field very weak, so that the effects might, if possible, be more marked:—

Volts at Terminals of Alternator when supplying the adjoining Current through respectively			Current in Alternator Armature.	Exciting Current.
Capacity.	Self-induction.	Non-inductive Resistance.	Amperes.	Amperes.
97·8	84·0	87·7	4·97	6·3
57·1	48·8	50 3	2 91	3·2
61·6	41·0	46·0	7·40	3·2
205·0	191·5	193·6	6·00	18·0

Corresponding impressed E.M.F. calculated for each of the above cases,			Current in Alternator Armature.	Exciting Current.
Capacity.	Self-Induction.	Non-Inductive Resistance.	Amperes.	Amperes.
91·5	91·6	92·0	4·97	6·3
53·5	52·6	52·7	2·91	3·2
51·35	52·5	52·0	7·4	3·2
198·0	200·0	198·5	6·00	18·0

Mr. Weekes. In the calculation no reaction on the field was allowed other than that allowed for in the self-induction of the armature, and the conclusion shows that for this machine, at least, no other correction is needed.

The general details of machine were as follows:—

Self-induction when fully excited, 0·00101 secohms;
Resistance of armature and leads, 0·75 ohm;
$\sim = 205$.

It must, however, be remembered that this alternator was a small one, and it may be that the effects would be slightly different for machines of larger dimensions.

Mr. Miller. Mr. Leslie Miller: There are two other points of advantage with this method. One is that you have full speed with a full field and the full current in the armature, and, therefore, the machine can be tested for heating effects when the ventilation is normal. One other advantage is that the centrifugal force is also the normal. Unless Mr. Mordey's plan is used it is impossible to get that. Tested in the ordinary manner, when the power at disposal is insufficient, the machine can only run at full speed with full armature current in a weak field, or not with full speed in a full field. In Mr. Mordey's plan the centrifugal force is correct, and you can tell if the wires show any signs of a tendency to shift on the armature. They may be held on by centrifugal force or may be tending to come away, according to the type of machine.

Mr. Mordey. Mr. W. M. Mordey: I am much obliged for the kind reception given to the paper, and I will go briefly through the criticisms that have been made. I know quite well the paper is not complete in all respects. If I had waited until I could fully deal with the whole subject, I should never have brought it here at all. Professor Ayrton referred to the method of circulating power, and says it does not give any indication of whether the shaft is strong enough. That is quite true. The method does not test the shaft except for weight-carrying; nor, so far as regards side pull of belts, does it give any information. But makers have a good deal of experience of shafts and bearings in all sorts of machines on which they are able to fall

back. Mr. Crompton has just told me, as an instance of comparatively frictionless running, that one of my alternators ran for 20 minutes after the belt was thrown off. Professor Ayrton thinks this method should be traced back to the plan of using a single transformer with one winding opposed to the other, due to Dr. Sumpner. I had some recollection of such a plan, but did not succeed in finding it. It now appears to be in a B.A. paper by Professor Ayrton and Dr. Sumpner, read in 1892 (*Electrician*, October 7, 1892). A third transformer was used to upset the balance between the two equal windings of a special 1 to 1 transformer. I am glad to refer to it as another example of circulating power. Mr. Mordey.

Dr. Thompson says the conditions in a machine running in the way I have suggested are not quite the same as when running entirely as a generator or as a motor. That is quite true. There are differences, but I do not think they are such as to impair the efficiency of the test, or any of the practical conditions the test seeks to determine. Dr. Thompson pointed out that a machine might be running in a station on transformers very lightly loaded, or not loaded at all, but merely on the primaries of the transformer armatures, and that in such a case it would be running simply on choking coils. He quotes me as having said I objected to use choking coils for testing an alternator on account of the great lag, and because the conditions are not then close enough to those of practice to make them of any value. There is, however, an important difference. Choking coils have a very small power-factor—that is, a large lag—while good closed-circuit transformers have a large power-factor. For example, the power-factor of good closed-circuit transformers on open secondary varies from about 0·75 to 0·9 (as Dr. Fleming has recently shown); so that, even if the alternator is running with its full current, on unloaded transformers the lag is small, and the actual power is 0·75 to 0·9 of the apparent power that is going out. That is pretty close to the conditions of its running with an actual load of lamps. The fact alluded to by Dr. Thompson that the curves of Fig. 6 meet on the zero line, if continued, is very interesting. I am glad

Mr. Mordey Dr. Fleming has brought his wattmeter before us; I think it will be very useful. We have had great struggles for want of accurate wattmeters. Until quite recently most of the results obtained from so-called wattmeters have not been accurate, although the principles on which they should be constructed have been fairly understood. I do not quite see how a very large load can be put on this particular instrument. I think Dr. Fleming ought to add some arrangement to it so that not only the range of volts can be varied as he proposes, but also the range of current; and that he should make it astatic by having two coils in each circuit arranged so that there can be no disturbing effect by any external field. That is a difficulty which Lord Kelvin has recognised in an instrument recently introduced. I think, in fact, that all instruments of the dynamometer type should be so arranged.

Dr. Fleming mentioned that alternate currents sometimes act as if they had a direct-current constituent; that is known to some of us who have worked with alternating currents. It is quite possible with suitable arrangements, without anything except ordinary stationary arrangements, like transformers, to get direct currents out of alternating currents; an alternate current can be made to do a good deal of electrolytic work. I have found by experiment that it is also quite possible to split up, by means of motors, alternating currents into direct currents, so that you can have a positive current going one way, a negative current the other way, the two joining again and going on as an alternating current. I should like to say more on this point, but must pass on.

Dr. Hopkinson was the next speaker. I think that he would find, if he looks at the early part of my paper, that I quite recognised the twofold advantages of his method—namely, the greater accuracy obtained over the old methods, and, of course, the greater economy. As to his criticism of the test applied to a single-current direct dynamo with two commutators, I quite agree that my test so applied is just the same as the test with a dynamotor with double winding, one half acting as a motor, and the other half as a generator. In the Brush dynamo the machine has two commutators to start with, but it has so few parts in the

armature that I do not think the method could be applied. Mr. Mordey. Something has been said as to the unequal effect on the fields of the two portions of the armature running, one portion as a motor and the other as a generator. If reference is made to Fig. 5, it will be found that some two years ago here I described an experiment with an alternator in which one coil was taken out of the circuit and merely connected to a voltmeter. The other coils were then loaded from nothing up to full load, and when the excitation was kept constant the volts shown by the idle coil remained constant. I took that to indicate that the reaction of the armature on the field was not appreciable under those circumstances.

Mr. Kapp followed Dr. Hopkinson, and I am glad he confirms the result given in my Fig. 6. I think I said in my paper that that result was really what one would expect if one gave any thought to it at all.

Mr. Harrison referred to Dr. Hopkinson's paper, and he very properly points out that I give Dr. Hopkinson credit for having prophesied some of these things. No doubt I should have been able to get on a great deal better with my work if I had been able fully to benefit by Dr. Hopkinson's papers. I expect a good many of us are not able, to use Mr. Harrison's figure, to chip off the mathematics and get at the really beautiful statue inside. I have had to work round as if the statue did not exist at all. We do not often get Dr. Hopkinson here now—I wish we saw him more frequently—but as he is here I may take the opportunity of uttering a wish that when writing the papers which always turn out long afterwards to be so very important, he would remember people like me, and condescend to our level: it would greatly increase the immediate value of his writings. Now Mr. Harrison comes from the theoretical to the practical, and he tells us some things which, I may say, astonish me a good deal. As to the remarks on alternators working as motors on circuits having a higher E.M.F. than the motors would give as generators, I think Mr. Harrison is complicating matters needlessly. For those who have not followed Dr. Hopkinson's treatment, a consideration of Fig. 6 will show a simple way out of the difficulty.

Mr. Mordey. The experiments on the variation set up in the field current to the Siemens alternator are certainly beyond my experience. There must be an enormous reaction in a machine which varies its field current in the way described. I have had a good deal of experience of alternators, and have tried to find the faintest difference of field current when the whole of the armature current was thrown on or off suddenly, and have never seen in my own machines the slightest flicker. I have noticed a pulsation in the armature current between two alternators, one used as a motor, but it has not been serious.

I am glad Mr. Swinburne has sent a communication, but think he criticises the modification of Dr. Hopkinson's method that I have brought forward, on grounds that are not quite fair. I was trying to test an alternator; I was not trying to test an engine or a belt, or anything else. Really the alternator has to be driven somehow, and for the purpose of my argument I may assume that my machine is direct-driven, and in that way I am perfectly accurate. I would also point out that I mentioned methods for separately getting the belt or gearing losses.

It is to be feared that the application of this method to multipolar direct-current dynamos will not prove a very convenient one, but as it has been suggested by Professor Ayrton and Mr. Swinburne, we shall be interested to hear of its being applied. As to running alternators at full current but with low speed, as recommended or suggested by Mr. Swinburne, I am afraid it is not of much use, as the losses go up very much quicker than the speed. For my part—and I suppose everyone will agree with me—nothing short of full speed, full excitation, full E.M.F., full current, and full torque can be taken as entirely satisfactory. The method I have given, in an almost complete way satisfies these conditions without the usual cost of power, and Mr. Swinburne's suggested alternative satisfies only one or two of them. Mr. Swinburne also referred to the parallel working part of the paper, and says that the small current cannot be obtained at breaking the alternator out of parallel in the way that I described. Perhaps not on paper; but it can be obtained in practice, and really that is what we have to consider. It is

necessary in reading these statements of Mr. Swinburne's to remember that when he says such and such is the case, he merely means that he thinks it is the case. Mr. Swinburne and Mr. Crompton referred to the question of engine governors. I quite agree with them, and with what Mr. Willans appears to have said—that the governors ought not to be used on individual engines with parallel working. But an examination of my paper will show that I have by no means overlooked this question. It is one which I and my colleagues have thought about and discussed a good deal. If you think of it, when all the alternators are in parallel the engines are clutched together through the alternators, and the governors are therefore, as it were, all driven from one shaft. If they hunt independently under these conditions, they must be bad governors, or badly regulated. I would go so far as to say that it ought to be possible to put a governor on the main steam pipe and have no governor at all on the individual engines, except, perhaps, a dynamometric governor, which would control the amount of steam and the cut-off to let the engine exert the necessary power. The speed ought to be controlled by a synchronous motor running on the mains acting on a centrifugal governor operating on the main steam pipe. Theoretically there should be one governor controlling the speed of the whole combination, for, as I pointed out above, a lot of engines and alternators coupled are really one machine, and the speed of that machine ought to be controlled by one governor. Mr. Mordey.

Mr. Bailey's method of finding out the weak points of the alternators at Paddington is interesting. I rather long to have a difficulty of that sort. We never have much difficulty in finding out the faults in our armature coils. In the machines at Paddington it was a different matter; it was the custom to short-circuit the coils if they had a fault in them—a plan that is not possible with modern machines. It is not generally known that at Paddington it was possible to regulate by short-circuiting the coils. You also may not know that with those extraordinary machines, the more lamps you put on, the less power the engines developed. That was a unique system for getting economy from a central station! The reaction in the machine was so great

Mr. Mordey. that it was only when the eddies were held under control by the reaction of the actual working current that the machines were fairly efficient. When running at light load the eddies had everything their own way, and the losses were enormous. In fact, I remember Mr. Spagnoletti saying if they took the wires out of the terminals, the engines could not turn the dynamos round.

I have to thank other speakers for their remarks, but think there is no other controversial matter that calls for a reply.

The President. The PRESIDENT: I have now a very pleasing duty to perform. We are always very glad both to see and to hear Mr. Mordey. Not only in his paper, but in his remarks, and especially in his summary of the discussion, we find a freshness that we all enjoy, a novelty that we appreciate, and generally we receive instruction by which we benefit. I am quite certain that what I am proposing now—viz., that a very hearty vote of thanks be given to Mr. Mordey for the treat he has given us—will be received by you with acclamation.

The motion was carried unanimously.

The PRESIDENT announced that at the ballot which had taken place for new members, the following candidates were elected:—

Foreign Member:

D. Farman.

Members:

Joseph Bevan Braithwaite. | Arthur Hoare.

Herbert Talbot.

Associates:

William Hibberdine.	Edward Mann.
John William Manley.	George Ralph.

Students:

Sidney Allingham.
John Richard Jesse Bowden.
Charles Brandeis.
Gordon Paton Clark.
Edward Herbert Cozens-Hardy.
George H. Cutting.
Henry Okazaki Fleetwood.
Herbert Henry Gresswell.
Mervyn J. E. O'Gorman.
Henry Herbert Pickford.
Edmund A. Robins.
Leonard L. Robinson.
John Severs.
Robert John Jocelyn Swan.

The meeting then adjourned.

The Two Hundred and Fiftieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 23rd, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 9th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Charles W. Durnford. | Edward T. Stuart-Menteath.

Mr. G. Driver and Mr. J. Hewitt were appointed scrutineers of the ballot.

The PRESIDENT: As you have all had sent to you copies of the statement of accounts for 1892, I presume you will take it as read, so that time shall not be unnecessarily occupied in reading it now. These accounts having been now submitted, if any member desires to ask for further information, I think I may say that the Hon. Treasurer or the Secretary will be very glad to reply to any questions that members may desire to put.

Sir DAVID SALOMONS: It will be within the recollection of many gentlemen who are here to-night that about two years ago I felt personally some hesitation, as one serving on the Finance Committee, to express my opinion as to whether we were solvent or insolvent, or, at any rate, as to what our surplus might be. In consequence of my remarks, the Council took into consideration, as you all know, the question of raising the terms of entrance into the Institution, as well as the subscription, feeling that we

gave benefits quite equal to those of many institutions which charged a great deal more than we did; and, fortunately, the alteration has met with very good results. I wish now to particularly call your attention to the fact that there is a balance of some £800 or £900 to the good, and I trust use will be made of this surplus worthy of the Institution.

No question having been asked,

The PRESIDENT moved that the statement of accounts as just submitted be received and adopted.

Professor HUGHES seconded the motion, which was carried unanimously.

The PRESIDENT: I will now ask Sir David Salomons to read his paper on

A NEW FORM OF PORTABLE PHOTOMETER.

By Sir DAVID SALOMONS, Bart., M.A., Vice-President.

If two nicol prisms be mounted in a tube and a source of light is observed through them, then the amount of light which will reach the eye is governed by a well-known law depending on the relative positions of the prisms to one another. The quantity of light passing depends upon a function of the angle which the polarising plane of the polarising prism makes with the polarising plane of the analyser. And since one half of the light issuing from the source of illumination observed will never reach the eye (*i.e.*, the ordinary ray which is thrown out by the polariser), the light which reaches the eye will be half the original light multiplied by a function of the angle included between the polarising planes of the two nicols. This function is the square of the cosine of the angle included between the two planes mentioned. Consequently, the quantity of light reaching the eye from a given source will be governed by the well-known formula, as follows:— Sir David Salomons.

Let I = intensity of the light observed,

„ L = light which reaches the eye,

„ O = ordinary ray in polariser,

Sir David Salomons.

Let E = extraordinary ray in polariser,
" θ = angle between the planes of polarisation of polariser and analyser,

the light which reaches the analyser is E, or $\frac{I}{2}$, since $O = E$ and $I = O + E$.

The amount of light which reaches the eye is expressed by the equation,

$$L = \cos^2 \theta\, E \text{ or } L = \tfrac{1}{2} \cos^2 \theta\, I;$$

hence L varies as $\cos^2 \theta$.

In this instrument two tubes are mounted as a binocular capable of being adjusted for distance apart, in order to suit the eyes of different individuals, and the hinge portion used for such adjustment is made without a stop, so that the position of the tubes may be reversed in its relation to the eyes. Each of these tubes contains two nicols, arranged as usual; but, instead of the analyser being revolved, the polariser is capable of being turned. The polarising plane of the analyser is set at 45° from the vertical in order to obtain the scales in a convenient position upon the tubes.

Consider for a moment one of the tubes—a description of which applies to the second. The polariser is so mounted that its position is indicated upon two scales at the same time; the one being graduated in degrees to be used for accurate work, and the other graduated in divisions indicating what proportion of the light observed will reach the eye. One scale is called the "degree scale," and the other the "light proportion scale." The latter scale is so graduated that it reads unity for the "light field," and zero for the "dark field." The intermediate division numbers are 0·1, 0·2, and so on. This scale is used for approximate measurement.

When the "light proportion scale" reads unity the "degree "scale" indicates 0°, and when the former shows zero the latter scale reads 90°.

The method of determining the positions of the divisions:—

$$\text{We have } L = \tfrac{1}{2} \cos^2 \theta\, I = \cos^2 \theta\, \frac{I}{2}.$$

Sir David Salomons.

Now equate ... ... $\cos^2 \theta = 0{\cdot}1$

$\cos^2 \theta = 0{\cdot}2$

and so on.

Solve each equation, and the values of the angles so found are the successive points on the scale required. The following table gives the angles by working to three decimal places in making the calculation:—

Divisions upon the "Light Proportion Scale."			θ.	
0	...	...	90°	no light passes.
0·1	...	...	71° 26′	
0·2	...	...	63° 34′	
0·3	...	...	56° 13′	
0·4	...	...	50° 14′	
0·5	...	...	45°	
0·6	...	...	39° 46′	
0·7	...	...	33° 47′	
0·8	...	...	26° 26′	
0·9	...	...	18° 34′	
1·0	...	...	0°	maximum light passes.

If the values for $\cos^2 \theta$ for the right-hand and left-hand tubes are A and A′, and the values of the lights observed with the right and left tubes are I and I′.

Since the photometer is balanced at the start, each eye receives the same amount of light, it is therefore correct to equate $L = A\frac{I}{2}$ and $L = A'\frac{I'}{2}$. It is evident the result will remain unaltered if we regard these equations as $L = A\,I$ and $L = A'\,I'$, and it will save the observer the trouble of doubling the final result.

In front of the polariser a small round opal, or some equivalent, screen is placed; and the light upon this screen is that which is directly to be observed. The screen is mounted in a cell and screwed on the end of the tube. A lens is inserted in the tube to focus this screen upon the retina. This screen may be removed. When desired, an adjustable lens may be added in

Sir David Salomons.

front of the observation tube to focus the light in the plane of the screen. In some cases this may be convenient.

To make an observation with this instrument it is placed before the eyes in the same way as a field glass, each polariser having been turned to the position of "light field," when the "degree scale" will indicate zero degrees, and the "light pro-"portion scale" will read 1. One tube is now directed towards the source of light under examination, and the other tube towards some known standard radiant. The distance of the light under observation, and the distance of the radiant from their respective screens (which are before the polarisers) are measured. Then, according to the usual methods of photometric measurement, assuming that the source of light under observation cannot be moved, it would be necessary to shift the standard radiant in order to get a balance—*i.e.*, to render the screen, observed by each eye, illuminated at the same intensity. Instead of shifting the standard radiant, the balance with this instrument is obtained by revolving the polariser, which faces the brighter screen (should any difference exist) in order to reduce the illumination of both screens to the same apparent intensity. The value of the light under investigation in terms of the standard may be calculated when the amount of light cut off by the polariser, the distances of the two sources of illumination in question, and the value of the standard illuminant, are known.

It should be again observed that, since the ordinary ray reaches neither eye, it is equivalent to an equation, each side of which is divided by two; and, consequently, the final result will be the same as if no such division by two existed.

To make such an instrument thoroughly practical several mechanical appliances are needed, and exist in this instrument. It will be a convenience if the standard radiant can be placed always at such a distance that the square of this distance shall be unity; and instead of this radiant being in front of the instrument—which would cause it to shed light on the other screen, or be in the way—it should be placed at the side, and illuminate its screen by means of a right-angled reflecting prism, and the arrangement is such in this apparatus.

Sir David Salomons.

The comparison radiant for this instrument consists of a standard candle contained in a tube; the candle being fed up by means of a spring, the top of the tube forming an abutment to keep the candle flame at one level although the length of the candle is always altering. This abutment piece is surrounded by a cup to catch any grease which may run over. The candle tube has a slot cut in it, in order that the amount of candle in the tube may be observed at any moment. The candle tube is carried in a light framework, which supports a metal cylinder that acts as a chimney to keep off draughts. This chimney has in it a small window, into which slides a slip of clear glass, and it is through this window that the light from the candle passes to illuminate its screen. The clear glass may be replaced by tinted glass when desired.

The chimney is carried in a special form of gimbals, so that no matter what inclination may be given to the binocular, the candle will always remain vertical, since the gimbals are attached to the binocular by means of an arm. The inner ring of the gimbals is in reality only half a ring, and the plane of the outer ring when the candle is vertical, instead of being at right angles to the axis of the candle, is oblique to its axis. This construction is necessary, otherwise the rings would come before the window in the chimney and so obstruct the light. In front of this window is placed a suitably shaped tube, capable of being lengthened and shortened, if required, and which fills up the space between this window and the reflecting prism; this prism being situated in front of the screen at the end of the tube containing the nicols. The object to be secured is that extraneous light should be kept from this screen. The weight of this adjusting tube is counterpoised so that the candle may always remain vertical. The arm connecting the candle with all its supplementary apparatus is carried upon its observation tube (which contains the nicols) by means of a ring arranged in such a way that, although removable, it cannot turn except to a small degree by means of an automatic motion. The reflecting prism also is carried in a similar way. The arm has two motions upon it—one a clamping joint to enable the candle flame to be brought in front of the prism to a nicety,

Sir David Salomons.

and the other a kind of parallel motion arrangement by which the candle can be placed to two given distances from the prism, according as the parallel arrangement (somewhat similar to a parallel ruler) is pushed to the one side or to the other. The measurements and the arrangements of the details are all so adjusted that the candle flame will always be facing the reflecting prism for all inclinations of the binocular, and the two distances to which the candle may be moved are so regulated that the apparent distances of the flame from the screen it illuminates shall be either the square root of 10 or the square root of 12. These two distances are the most convenient, one for use when measuring by a decimal system, and the other when measurements are made in feet.

It may be mentioned that the candle arrangement with its arm can be removed at pleasure, so also can the reflecting prism. It will be noticed that the weight of the candle with its apparatus tends to twist the ring round upon the tube containing the nicols, and it might appear that this could be avoided by simply cutting a slot in the ring to push over a pin screwed into the nicol-tube. This, however, is not feasible, for, in order always to have the relative positions of all parts as they should be, it is essential that the ring, which carries the candle and its arm, should have a slight circular motion. This is obtained by placing upon the direct observation nicol-tube another ring, which is connected with the candle-arm ring by means of a slotted sliding bar in such a way that the candle apparatus cannot be displaced and the relative positions are always correctly maintained.

A description of this part, simple as it is, would be complex, so that references must be made to the plates attached hereto, by the inspection of which the arrangement will appear clear. The eyes of any one person differ, so that an observation error is sure to be introduced. In order to eliminate this error, the instrument is so arranged that the relative positions of the parts of the instrument can be reversed when a second observation is taken, which is equivalent to an observation with the eyes reversed. A mean of the two observations is then taken. The instrument described has this particular advantage not possessed by other photometers.

Sir David Salomons.

At starting it is only necessary to place the candle in its fittings on any of the photometers in general use, in order to ascertain the standard value of the light which issues from the window and from the tube when the latter is used. These two values, which may practically be considered as constants, can be engraved upon the chimney. The loss of light due to the reflecting prism may be ascertained at the same time.

When it is desired to put the instrument (set up and the candle alight) upon a table, it is placed upon a small slender tripod. This tripod, which can be closed to render it portable, accompanies the instrument. It may often be found convenient, when using this apparatus, to place it upon a tripod after the same manner as a theodolite.

This photometer packs up into a sling case barely larger than those used for field glasses.

Some of the advantages derived from this instrument may be stated as follows:—Very powerful arc lights may easily be compared with one another or with a standard source of light. It may be used for measuring various sources of light, such as daylight out of doors with light in a room; and for the estimating of photographic exposures. That the instrument is exceedingly convenient, compact, portable, and easy to use, need scarcely be stated. It may also be employed for rapidly comparing the light in one or more places far apart, which is frequently a great convenience.

Two points alone might be urged against the principle. The first is that nicol prisms are not, as a rule, sufficiently accurate for very close measurements. However, to make these prisms quite accurate, or at any rate sufficiently so for this purpose, is no difficult matter, as may easily be proved by taking a number of nicol prisms at random and testing them. Secondly, errors are introduced due to internal reflections as well as reflections which take place at the surfaces of the prisms. These losses occur for the nicols in both tubes, and when the losses in each tube are deducted from one another for any positions of the nicols, the difference will be found to be so small as to affect to an inappreciable extent the result.

Sir David Salomons

The nicol prisms in the tubes might be dispensed with and be replaced by adjustable diaphragms somewhat similar to the "Iris" pattern, one being placed in each tube and an index scale showing the area, which is free, for the light to pass; as well as a proportion scale for reading the quantity of light which passes without calculation. The principle, of course, would remain the same as that already described.

Again, it might be possible to vary the proportion of light passing down the tubes by wedges of tinted glass; but on the whole the nicols appear to present so many advantages that a preference was given to their use.

Appended hereto are the formulæ applicable to this apparatus for general work, and it will be unnecessary to refer to any tables or to make tedious calculations, excepting in those cases where great accuracy is required, in which event the scale of degrees must be used and the values of the cosines be looked out and squared.

Let X = the light under examination.
,, l = the comparison light expressed in standard candles.
,, D = the distance in inches of X from the screen of the photometer facing X.
,, d = the distance in inches of l from the screen of the photometer facing l.
,, θ = the angle between the polarising planes of the nicols in the tube facing X.
,, θ' = the angle between the polarising planes of the nicols in the tube facing l.

Then
$$\text{Cos}^2\,\theta\,\frac{X}{D^2} = \cos^2\theta'\,\frac{l}{d^2},$$

and
$$X = \frac{\cos^2\theta'\,D^2}{\cos^2\theta\;\;d^2}\,l.$$

Let
$$\text{Cos}^2\,\theta = A \text{ and } \cos^2\theta' = A'.$$

Then
$$X = \frac{A'}{A}\,\frac{D^2}{d^2}\,l.$$

In this photometer d may be made $\sqrt{10}$ or $\sqrt{12}$, so that the last equation may be written,

Sir David Salomons.

$$X = \frac{A'}{A} \cdot \frac{D^2}{10} l \text{ or } \frac{A'}{A} \cdot \frac{D^2}{12} l,$$

according to the value of d.

If D is divided by 12 the distance will be expressed in feet.

Let
$$\frac{D}{12} = D'.$$

Then
$$X = \frac{A'}{A} \cdot \frac{D^2}{12} l \quad \dots \quad \dots \quad \dots \quad (1)$$

when distances are given in inches,

and
$$X = \frac{A' \, D'^2 \, l}{A} \quad \dots \quad \dots \quad \dots \quad (2)$$

when distances are expressed in feet.

When the values of A and A′ are calculated from the "degree "scale," certain special cases occur which simplify the equations. They are when $\theta = 0°$, $\theta' = 0°$, $\theta = \theta' = 0°$, $\theta = 45°$, and when $\theta' = 45°$. In these cases A and A′ will become unity for 0° and ½ for 45°.

With most observations the value of A′ will be 1, and equations (1) and (2) will then become,

$$X = \frac{D^2 \, l}{10 \, A} \quad \dots \quad \dots \quad \dots \quad (3)$$

and
$$X = \frac{\quad}{A} \quad \dots \quad \dots \quad \dots \quad (4)$$

When the values of A and A′ are obtained from the "light "proportion scale," the values are in decimals of one place, so that these figures may be read as whole numbers multiplied by 0·1.

Equations (3) and (4) may then be written—

$$X = \frac{D^2 \, l}{10 \, A \times 0{\cdot}1} = \frac{D^2 \, l}{A} \quad \dots \quad \dots \quad (5)$$

and
$$X = \frac{D'^2 \, l}{A \times 0{\cdot}1} = \frac{10 \, D'^2 \, l}{A} \quad \dots \quad (6)$$

Putting these equations into words:—

Rule 1.—When only the light under observation is reduced by turning the polariser, the "light proportion scale" is used, and the distance of this light from the screen is given in inches, then the square of this distance multiplied by the standard comparison light in candles, and this product divided by the reading on the

Sir David Salomons.

"light proportion scale" (read as a whole number) gives the value in standard candles of the light observed.

Rule 2.—When only the light under observation is reduced by turning the polariser, the "light proportion scale" is used, and the distance of this light from the screen is given in feet, then the square of this distance multiplied by 10 and by the standard comparison light in candles, and the products divided by the reading on the "light proportion scale" (read as a whole number) gives the value in standard candles of the light observed.

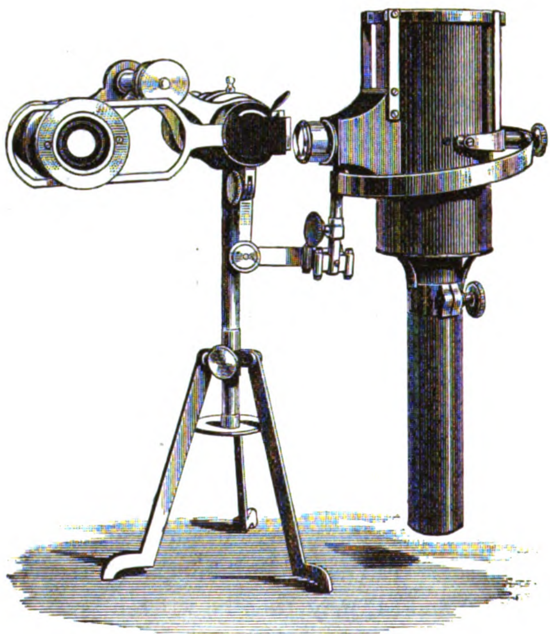


FIG. 1.

Fig. 1 shows the instrument set up for use upon the tripod. On the right is shown the candle-tube, with chimney and screening tube, set in its gimbals, the whole supported by the arm which connects it with one tube of the binocular. The tube of the binocular nearest the candle has the reflecting prism upon it. The end of the other tube shows the opal screen. The long link embracing this tube and connected with the other one, is the portion which gives the automatic adjustment to the reflecting

prism and candle arm. The distancing arrangement for the candle is seen at right-hand end of the candle-arm. Sir David Salomons.

When the photometer is used it is lifted off the tripod.

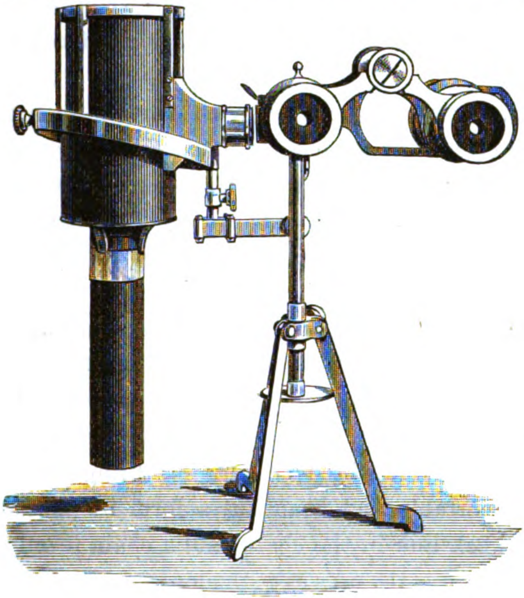


FIG. 2.

Fig. 2 is the reverse view of the last one, and shows the appearance of the instrument as seen from the side which faces the eyes.

Fig. 3 shows the arrangement of the binocular portion of the photometer diagramatically. At the top is shown the scale of divisions of the "light proportion scale." In the centre the end view of binocular is illustrated, showing on one tube a screen, and on the other tube the reflecting prism covering its screen.

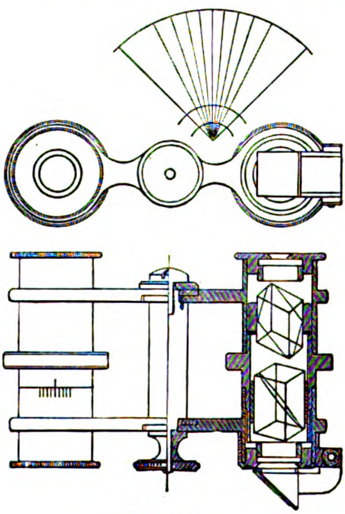


FIG. 3.

The lower diagram is a side view. On the left tube is seen

Sir David Salomons. one scale, the second one is on the other side. The centre portion is the hinge with a clamping nut, and the right tube is shown in section, with the reflecting prism attached.

The PRESIDENT: Perhaps some member would like to ask Sir David Salomons for some further information before the discussion commences.

Mr. Ovens. Mr. J. L. OVENS: I should like to ask whether the candle-power of the electric or other light under test in comparison with the candle used can be read off the instrument at once. In comparison with the unit of light, can a test be made of the light direct to show what power it is? How many units in comparison with the standard?

Sir DAVID SALOMONS: Yes. You can take this and look at it; I have taken the candle off just for convenience.

Mr. Crookes. Mr. CROOKES: There is one point about which I should like to ask Sir David Salomons. We are all much interested in this subject, and greatly indebted to him for bringing before us so compact and portable and beautiful a form of photometer; but there is one weak link in the chain, and a chain is only as strong as its weakest link. He uses as a standard a candle. Now a candle is the most imperfect standard we can have. No two standard candles agree. I think I am within the mark when I say that the variations are quite 10 per cent. between one set of standard candles and another; and if Sir David Salomons could apply his great ingenuity and mechanical powers to giving us some electrical standard which would always be the same, knowing the volts and amperes put through the lamp, it would increase the value of his photometer ten-fold. I know candles are used very largely for photometric purposes, but they are admitted to be very imperfect; and I think electrical engineers might devise something very much better and more trustworthy, for the difficulties are not at all too great to be got over if attention is devoted to the subject.

Mr. Trotter. Mr. A. P. TROTTER: I feel hardly competent to offer any remarks upon this subject, as I have not had the pleasure of reading the paper. Still less can one criticise an instrument until one has worked with it. I remember Captain Abney calling

attention some time ago to the disadvantages of eye-piece instruments, and the great superiority in photometric work of instruments at which you can look from a distance with both eyes, to take a general idea of the difference, which is a very slight one. It seemed that the half-image principle commonly employed in polarimeters could be applied to this. Looking at the one eye-piece only, you would have the light reflected from this prism through one half of the telescope, and the other half is compared with it. Candles are very bad as standards. But in a portable photometer we only want a secondary standard to carry about and to compare with the standard we keep at home. In the same way, a foot-rule is not a real standard, but it is used in the hope that someone has checked it at some time or other against a real standard. A glow lamp for a portable photometer ought to be tested before going out on a photometric expedition, and again on coming back. With a candle there is difficulty with draughts; and if you have a screen to shield it, the screen itself would alter the draught and the temperature, and thus alter the light of the candle. Mr. Trotter.

Professor W. E. AYRTON: I am sure we are all very glad to welcome any addition to our instrumental means for carrying out photometric experiments—an operation which is always attended with a certain amount of difficulty and uncertainty. But, like Mr. Trotter, I must confess I feel some doubt, in the absence of practical experience with the photometer before us, whether you can satisfactorily compare two things, such as two lights, when you are looking at one with one eye and the other with the other eye. The two eyes will give you one appearance, and it would, I fear, be very difficult to know which of the two lights was the brighter. Of course, if you are looking at two distinct things with one eye or with two eyes, then you can see which is the brighter; but when looking at two different things with two different eyes, would it not, as Mr. Trotter has so clearly pointed out, be very difficult, without a great deal of skill and practice, to make an accurate comparison? Professor Ayrton.

The question of the candle, too—a candle shaded by a screen, and a candle which I imagine from the apparatus is pushed up by

Professor Ayrton.

a spring through the nozzle,—I do not know if that is the arrangement; is that the arrangement? [Sir DAVID SALOMONS: Yes]—I have found to be very uncertain as to its illuminating power. Some years ago we tried in my optical laboratory, thinking it would be a very easy way of keeping the light of the candle at a fixed height, to use the old plan employed years ago by Palmer of pushing a candle up with a spring through the nozzle; but we found it did not answer at all, because the light varied very much from time to time with such an arrangement. In fact, to use a candle with any certainty, the whole of the upper part ought to be free from any surrounding screen, and certainly the wax must be free in the neighbourhood of the flame, so far as my experience goes, from any piece of metal.

There is another thing that it is very important to be able to do with a photometer, and that is, when you are comparing lights of very different colours, to be able—as was pointed out some years ago by Captain Abney—to vary the intensity of one of the lights rapidly backwards and forwards. You move the candle, or it may be the photometer screen you move—you move something or other—you vary the adjustment, making your standard first too bright, then too dark—you oscillate backwards and forwards about a mean, and by diminishing the amplitude of the oscillation you are able to finally decide that a certain red light, for example, is equal in brightness to a blue light. It would be difficult to use this method if the adjustment could only be varied slowly. Possibly, however, you could oscillate the adjustment in Sir David Salomons's apparatus sufficiently quickly to attain the same end. But an important thing to remember in designing a photometer is this desirability of being able to rapidly alter the luminosity of either the standard or the light you are trying to measure.

I quite agree with what Mr. Crookes has said about the uncertainty of the candle, not to mention the spring and nozzle business which I have spoken of, and I feel strongly that the glow lamp forms a much better standard of light. There is no necessity, however, to use an ammeter and a voltmeter; it is quite sufficient to use an ammeter. If you observe the current through your lamp you are able to know the brightness of the

light; and I may take this opportunity of mentioning that I have had experiments going on in my optical laboratory for some years past to see whether lamps run at very much below their normal brilliancy constitute sufficiently definite standards of light when supplied with a constant current. I do not propose now to state how far we have attained perfect constancy, but the experiment we have been trying is to see whether the light given out by a lamp when a perfectly definite current is sent through that lamp, and when it is never raised to anything like its normal brilliancy, so that there is no fear of the glass being blackened,—whether you can say for years, or whether you cannot say for years, that that light will be a perfectly definite thing. I am in hope that the result will be Yes, though the experiments have not yet been carried on for a sufficiently long time to enable me to get to speak quite definitely. Suppose the answer turns out to be Yes: then with a certain glow lamp, and with a certain definite current passing through the filament, you will have a definite illumination. It is very easy to measure with perfect accuracy a particular current by means of a set-up ammeter—that is, an ammeter in which the pointer moves from one end of the scale to the other for a very small change in the maximum current—an instrument, therefore, which reads one current with extreme accuracy, and reads no other current at all—an ampere gauge, let us say, rather than an ammeter. With such an ampere gauge you will know with great accuracy whether the current passing through the lamp has a certain value, and thus you will know with similar accuracy the light given out by the glow lamp. I hope in that way to supply a practical standard of light which will have none of the disadvantages of the standard candles, which will not be affected, of course, by wind, and which will give you exactly the same light whenever used. Professor Ayrton.

Mr. J. S. FAIRFAX: I have been making some photometric experiments for the last two years upon daylight, measuring the light coming through two windows, both facing the north, with a pier of brickwork 3 ft. in width between the two windows, and I have found that the daylight itself varies according to circumstances within 10 seconds; so that although it is a north light Mr. Fairfax

Mr Fairfax. (which is supposed to be extremely steady), yet if there are clouds passing across, say from east to west or from west to east, the light will vary within 10 seconds. Usually, if the wind is blowing rather hard, it will be about 15 seconds, but if the wind is extremely high it will be within 10 seconds. I have been doing this with a very simple arrangement—so simple that I take my breakfast while doing it. It is so portable that I have it in my pocket: I thought it would interest you to see it. It is just a silver napkin ring, and you can compare the colours of two or more different lights at one and the same time. In addition to that, you can compare artificial lights with daylight; so it seems to me it would offer very great advantages in the case of making experiments with any particular standard of light. I am quite sure that we have never yet attained to anything that will do for the standard of light that we can use conveniently and at call, and at our own requirements. Mr. Lovibond, who invented the tintometer, told me that the only white light he could get was that of a sea fog. We cannot command a sea fog when we wish to make experiments; but when we find that daylight itself varies so quickly, say between 11 and 12 o'clock in the morning, and will show, say from one window in the same wall a distinct golden tint, and from the other a whiter light, and within 10 or 15 seconds changes or is reversed, I think that shows that we have no standard we can go back upon to show comparative colour. Daylight is the light we like to judge all others by. Candle-light must necessarily be very imperfect: for example, a defective wick may cause the candle to burn more quickly at some than at other times; any spring to force it up, or metal to carry away heat, varies the pressure on, or the melting point of, the candle; and so on. But I will just leave this on the table, so that you can see for yourselves.

Sir David Salomons: Will you show us how you use it?

Mr. Fairfax: Yes. You simply put it on a white surface. The various rays of light are reflected from the wall of the ring and fall as cardioid curves on a white horizontal surface placed beneath. The main point to be observed is that either the photometer or the source of light should always be at a certain definite angle, so that when measuring candles, one being at a greater

distance than the other from the photometer, the angle should always be the same. Mr. Fairfax.

Sir David Salomons.

Sir DAVID SALOMONS: It seems to me that the discussion has turned chiefly upon the candle, which is the part to which I attach small importance, for the reason that I know this standard is very imperfect. For a portable apparatus it is the most convenient thing you can take about; and in order to limit the errors as far as possible, I have arranged to cut down the light, something after the Methven standard method, and to encase the candle with metal fairly close, so that a draught may be started very soon after it is lit. When the candle is tested on a photometer in the first instance, the value is taken when the temperature of the apparatus becomes a steady value; and in practice the candle is lit a few minutes before being required. True accuracy may not result, but close approximation may be obtained. Incandescent lamps no doubt would be far better, but you cannot carry them about in your pocket with a secondary battery unless the apparatus is extremely small, and then there is the chance that the acid may escape.

One point depends very much on the physiological construction of the individual. Professor Ayrton may remember I asked him what would be the best to use in the ordinary photometer of the grease-spot form, and he suggested an arrangement; but I have never to this day—and I daresay there are others who agree in this—been able to decide in my own mind when I have got a correct balance. I would ask Professor Ayrton to take this instrument and adjust it to a light, and he will find that the balance can be rapidly altered to and fro by a very small turn of the milled ring which carries the polariser; and it wants great delicacy to get the exact adjustment. You have the advantage for examining coloured lights in consequence. The half-image method, mentioned by Mr. Trotter, is a very good one, and is now beginning to be much used. I have tried it with prisms arranged for that purpose, and this instrument can be arranged to bring the illuminated discs close together, or half-discs if so desired. Lord Kelvin, when looking through this photometer, saw only one disc, the two superposed: the axes of his eyes

Sir David Salomons. would seem to incline differently from that of most people. Some here to-night see the discs in this way. By making the lenses slightly prismatic, the difficulty would be overcome. The reason why one image is seen in a binocular field glass is due to the fact that the object you are looking at is very far off; in the stereoscope, because the object observed is near, the lenses are prismatic in form, or some other device is employed to render the images coincident. I therefore assume that in 99 cases out of 100, in looking through binoculars at close objects, two images, and not one, will be seen. A Jellet prism will do what Mr. Trotter suggests—that is, halve the discs, as it were, to get the balance, like a biquartz employed with polarised light—but it is almost impossible to get a Jellet prism true. It is more a prism of theory than a prism of practice. The way that suggested itself to me as the simplest to bring about this result was to construct the instrument as it is here, and to bring the discs close together by using prismatic lenses.

The PRESIDENT: It is my pleasure now to propose to you that we accord to Sir David Salomons a hearty vote of thanks for the extremely interesting paper he has read, and the very pretty instrument that he has submitted to us to-night.

The motion was carried by acclamation.

The PRESIDENT: I now call on Mr. Walker to read his paper on "Earth Currents in India."

EARTH CURRENTS IN INDIA.

By E. O. WALKER, C.I.E., Member.

Mr. Walker. Papers on this subject were published in the *Journal* for the years 1883, 1884, 1888, and 1891. More recent observations, in connection with those previously taken, have led to an identity being revealed between the variations of these currents and those of atmospheric pressure. It is thought to be of interest, as the comparison is novel, to give a brief account of the facts which seem to establish this identity. In doing so references must necessarily be made to the former papers. In the study of this

subject much assistance has been afforded by the records kindly placed at my disposal by Mr. C. Chambers, F.R.S., Superintendent, Colaba Observatory; Mr. Michie Smith, F.R.S., Government Astronomer, Madras; and Mr. E. Kitto, F.R. Met. S., Superintendent, Falmouth Observatory; and by information furnished by Mr. W. Ellis, F.R.A.S., Greenwich Observatory; reference has also been made to the Bombay Meteorological Observations of 1845, and to the *Encyclopædia Britannica* (9th Edition, Vol. XVI.—" Meteorology "). Mr. Walker.

The observations in India were not as a rule taken with a sensitive galvanometer, for the currents were of sufficient magnitude in the localities in which observed not to require it. The use of a sensitive galvanometer involves complications from weak electro-chemical and thermal currents produced at junctions, and the magnetic system is subject to displacement from terrestrial magnetism, the effects of which are difficult to screen. In all delicately suspended magnets with long indicators, or with reflectors and distant scales, these effects are considerable.

It is necessary to remark that the long periods of settled weather in India are favourable to the regular recurrence of the same variations in meteorological phenomena from day to day during those periods; and this has been found to be the case with the earth currents. For fully 30° north and south of the equator the lines of maxima and minima run north and south, but in higher latitudes these directions are changed. Again, the amplitude of the oscillations of atmoshperic pressure is greater in equatorial than in higher regions, and, so far as can be ascertained, the earth currents have higher electro-motive forces in the former than in the latter. On the whole, therefore, India offers a better field for observation than the British Isles, where the weather is subject to such large and frequent disturbances from often unknown and unforeseen causes.

In my papers of 1884 and 1888 reference has been made to difference in elevation between two places being favourable to the production of an earth current, and to the probability that at any time difference in atmospheric density is also effectual in this way. Condensation and dispersion of aqueous vapour at high

Mr. Walker. elevations have by other observers been shown to affect the direction of the current. On the coast of India the disturbances to telephonic communication, when earth has been used, have been very great on long circuits during that part of the day when solar heat was most active, and when the sea-breeze prevailed, the cause apparently being, that electrically charged films and particles of vapour were driven across the wires and discharged into them. The effect was to produce a hissing or grating noise on the telephone which often drowned the voice of the operator. It is thought, then, that these considerations, coupled with the results of fresh observations which follow, may lend colour to the supposition that the movements of aqueous vapour, as evidenced by the daily barometric oscillations, are active in the production of earth currents.

Observations of earth currents have been taken on some days in March and April, 1891, between Belgaum and Vingorla; in February and July, 1892, between Madras and Vellore; in June and July between Paumben and Manaar; those of September and October, 1887, between Calcutta and Allahabad, also illustrate the same features of the daily recurrent phenomena. It is found that almost always the current flows in the morning from the inland places of observation to those on the sea-coast. A sketch map (Fig. 1) is appended to show the geographical positions of the places indicated. In the afternoon the direction of the current is reversed. On the sections of lines from Belgaum to Vingorla and Madras to Vellore, currents of high electro-motive force are observed from a half to five volts; they are remarkably steady, and cannot be produced by electro-chemical or thermal influences at the earth plates. Some observations kindly taken on one of the Madras-Penang cables in July, 1892, by Mr. Wolfe, acting-superintendent at Madras of the Eastern Extension Telegraph Company, also showed that electro-motive forces of from 0·1 to 14·4 volts were operating from 6.37 a.m. to 9.11 a.m. (between which hours observations only were made), and go to strengthen the conclusion that comparatively high forces do prevail about the time of the morning maximum of atmospheric pressure. The curve which shows the changes from early morning to evening

was given in Plate I. of my paper of September, 1884, and this is reproduced here (Fig. 2). These diurnal phenomena are occasionally reversed in times of cyclonic storms. The cables from Madras to Penang give evidence of similar effects. Mr. Walker.

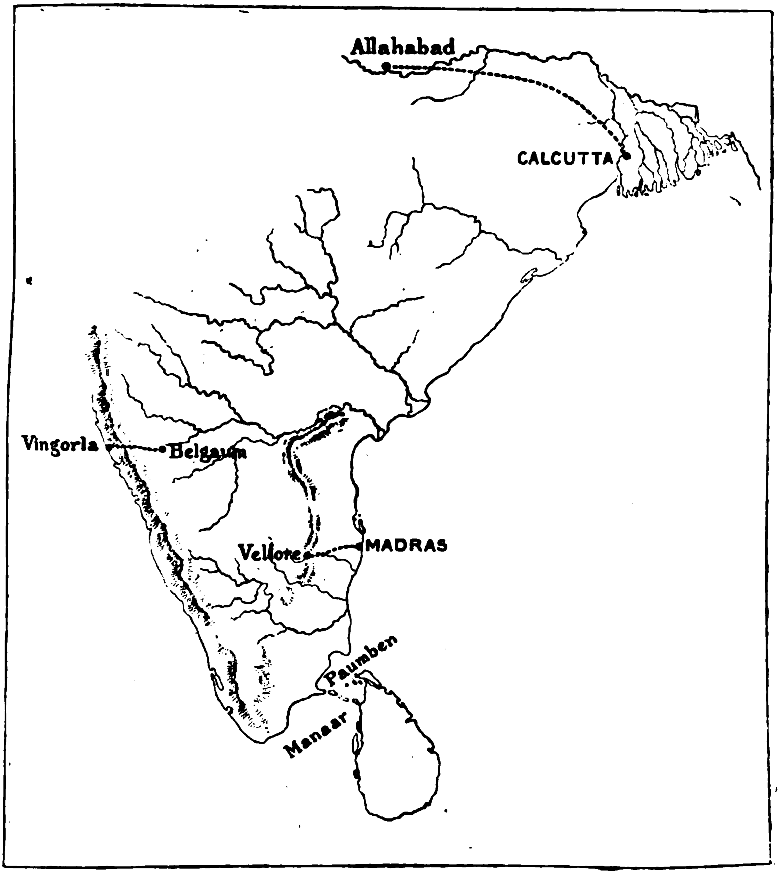


FIG. 1.

The observations taken at Paumben on the line to Manaar are very instructive: they comprised seven days, the current was very weak, and exhibited no regularity in ebb and flow. The line is about 50 miles in length, with a cable in circuit, and runs east and west. The difference between the phenomena exhibited here and those of Vellore and Belgaum I attribute to the fact that there can be but little difference in the meteorological conditions

Mr. Walker. of Paumben and Manaar, and barometrical oscillations of the same sign and amplitude occur synchronously at the two places. It is

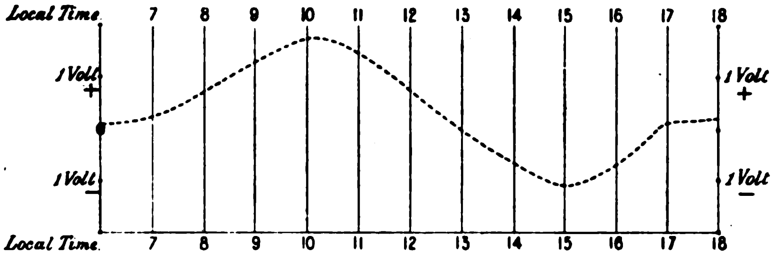
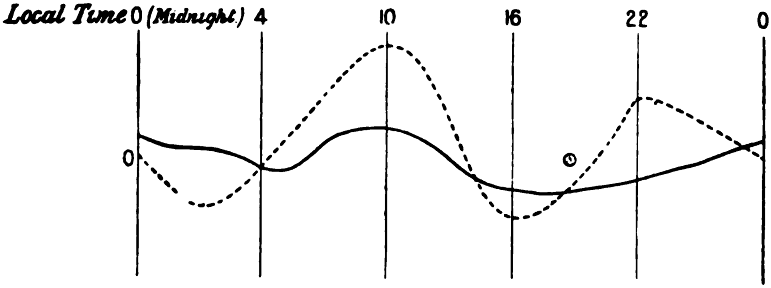


Fig. 2.

to be regretted that this supposition cannot be confirmed by records. It should not be omitted, however, to note that Paumben is only thirteen miles south of the magnetic equator, and it is possible that magnetic forces there are variable to a greater extent than at the other places noted above. The photographic records of the earth current at Greenwich Observatory in the line running east and west, prior to the disturbance which is caused by the City and South London Railway currents, show a rise and fall in the earth-current similar to that exhibited in Fig. 2. herewith, although in a circuit only three miles in length the electro-motive force is very small.

The barometric curves in Europe and in India as regards the diurnal oscillations are similar in respect of the morning maximum and afternoon minimum. Opportunity has been taken to verify this assertion by comparison of the Bombay curve with that of Falmouth. The general characteristic of the latter is naturally much obscured by the fitful weather to which this locality is subject. But on still days this similarity is undoubted. The morning maximum occurs from 9 a.m. to 10 a.m., occasionally as late as 11 a.m., and the afternoon minimum from 3 p.m. to 5 p.m. In Bombay, on three days in July taken at random, maxima occurred at 10 a.m., at 8 a.m., and at 11 a.m., and the minima on the same days at 5 p.m., 4 p.m., and 3 p.m. This variability in times of maxima and minima also characterises the earth currents. The curve for Vienna, as given in the *Encyclopædia Britannica*, is shown with that for Bombay

in Fig. 3, the two being typical to a large extent of the diurnal barometric oscillations in temperate and in tropical regions. Mr. Walker.



Curve at Bombay ------
" " Vienna ——

FIG. 3.

Could comparisons be instituted between the electro-motive forces of the earth currents in those two regions, it is not unlikely that the typical curves would be similar to these. In India, at least, a large number of observations warrant the belief that the barometric curve is also correspondent to that of the earth current in its rises and falls and change of direction. Experience at present only permits me to speak of the morning and afternoon changes. It has not been possible in most localities here treated of to secure observations during the night.

Fig. 4 shows the mean daily variation in declination in Madras for the month of July, 1855. Tables for later years are not available. But independent observations recently made show that the variation at present is very similar. It is preferable to give the curve constructed from the Madras Observatory tables, which contain the indications of exact instruments. The declination curve for the 6th July last, furnished to me from the Colaba Observatory, Bombay, bears the same character. It would seem, on the whole, that the earth current tends to maxima when the diurnal variations in declination are at their minima. If this be a fact, it is one of great interest, but it cannot be accepted without more prolonged observations in different localities.

The earth-current curve is by no means a smooth one, and

Mr. Walker. the rate of oscillation is a factor which is of great importance in forming a correct conclusion as to the origin of the current.

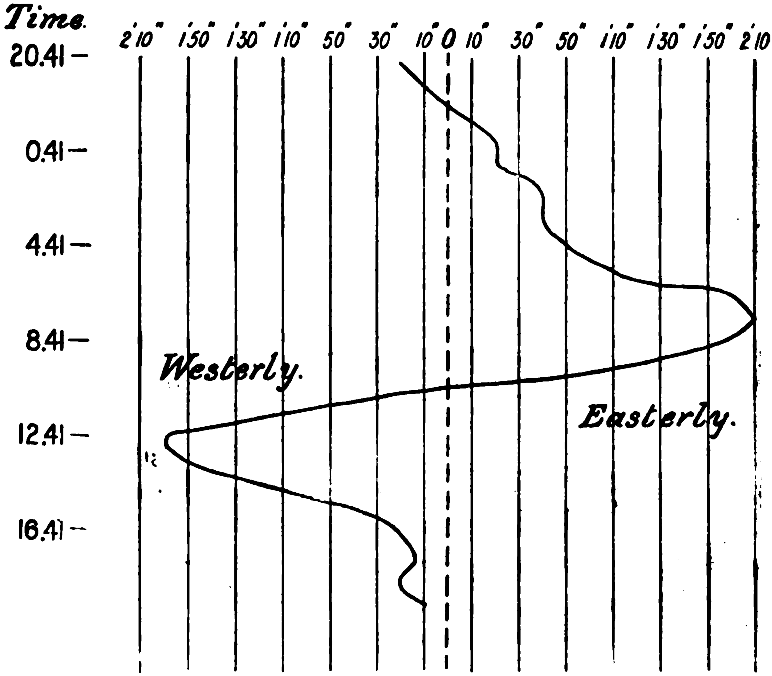


Fig. 4.

The time of change of the needle of the galvanometer through several degrees, while on the move on quiet days, has therefore been observed. The motion on such days is slow, and shows on the occasions in question rates of change of force in four observations of 5·7, 9·0, 13·5, and 17·1 feet per hour only. In some similar observations made in Calcutta in 1887, a mean rate of only 0·5 foot per hour was ascertained. Be the cause of the current what it may, it may safely be asserted that it operates slowly on quiet days. This feature of the phenomenon is well worthy of further study.

It will not be going too far, it is thought, in the light of the above facts, to surmise that the alteration in inductive capacity of the atmosphere by the action of solar heat occasions a slow redistribution of static charge on the earth's surface, the

aqueous vapour being the agent in this gradually progressive change. The uprisal of vapour under the influence of the sun in the early hours of the morning, and the condensation and fall in the afternoon, are in the East, and often in Europe, the phenomena of each day, and influence the barometric curves. If, as observation seems to show, high potentials are met with in the upper regions, these ascending and descending columns of vapour must be very potent in bringing successive portions of the earth's surface within their influence. So far as can be seen in India, the tendency in the morning is apparently to an inflow of negative electricity from sea-coasts to places inland where the barometric oscillations are large, and in the afternoon to an outflow. Thus, if there be induction from high regions, the gradual ascent of vapour in the air through heat promotes it, but the re-condensation later in the day of vapour operates to reduce it. Mr. Walker.

It is felt that, in venturing upon this hypothesis, the cause of perturbations cannot be entirely satisfactorily dealt with. These are often so rapid in their changes that they fail to be registered in our observatories by photography, and are often far stronger than any forces measured on quiet days. During such disturbances the barometric trace scarcely varies from an even line, though in some cases minute oscillations of the mercury can be detected. It will be probably necessary to look for the cause of these violent changes to increased solar activity, evidences of which are, in fact, often seen at such times. If electrification in high regions be due directly to the sun, the surface of the earth must partake of modifications of that condition.

After reading of Mr. Walker's paper,

Mr. ALEX. J. S. ADAMS: I should like to have had a copy of this paper earlier, in order that I might have studied the results brought before us; but it really seems to me that the author has been dealing, if his results with regard to the effect of vapour are correct, with local disturbances. There is, however, this fact—and I think it will be admitted by all who have studied the question deeply—that variations of the earth current, like those of the Mr. Adams.

Mr Adams. magnetic elements, in any one place, find their counterpart, in degree, and in sequence of local time lunar and solar, in every other part of the earth's surface; and if these Indian variations are the ordinary variations, then they certainly belong to changes which take place over the world as a whole.

Now the diurnal inequality variation occurs, day by day, in both magnet and earth current; and if the barometer variations at Greenwich be examined, they will be found to assimilate to this inequality; and this diurnal inequality in all three may, I think, be shown to be a true tidal movement, having a system of double progression: hence I am not surprised to learn from the paper that in India the barometer variations and those of the earth current largely agree.

We must remember, however, that in these things we are dealing with parts of a grand whole, and not with local effects. I have heard it said that such a snowstorm has produced this, and a change of wind has resulted in that, but never have I found one iota of proof for either.

Although I cannot discuss this paper so fully as I should like, still, having laboured at the subject for about 30 years, I may perhaps be permitted to say how the case, as regards earth-current variation, stands in my own mind at the present moment. I wish not to dogmatise. I will say I believe the matter to stand thus: In 1883 I sought to show that the earth-current variations take a tidal form; and I repeat to-day, after many more years of research, that they take a tidal form—diurnal inequality, double tide, spring and neap tides, solar and lunar declination tides, semi-annual and annual tides—although, like those similar ones of the ocean waters, they are interwoven with each its own period and power.

Now, with regard to the coincidence between the variations of atmospheric weight and those of the earth current, there is in it, to my mind, further evidence in support of my contention that these variations are the results of variations in gravitation.

There is plentiful evidence that gravitation is varied by sun and moon, and as a result of that variation in gravitation that we get our variation of the earth's electrical and magnetic conditions.

Although I may stand alone in this view of the matter, I think that a few years will suffice to prove it to be correct. Mr. Adams.

I have been particularly interested in the paper read to-night, but really do not think that vapour in given localities had anything to do in bringing about the results reported.

Sir David Salomons.

Sir DAVID SALOMONS: As I have had no experience of earth currents, except in the most superficial way, I merely wish to point out a matter which Mr. Walker perhaps already knows. He has mentioned in his paper—almost the last words—that the solar heat has something to do with these earth currents. By this remark I do not know whether he means the sun being obscured by earth, clouded, or whether he means the differences of temperature at the surface of the sun; he does not state which. I would like him very much to read, as I daresay many here have done, the recent address of Lord Kelvin to the Royal Society, in which he gives most interesting reasoning to show that the known differences of temperature at the sun's surface would not in any way influence the earth as regards magnetic currents. I merely call attention to this in order that Mr. Walker may explain which way we are to understand his remarks.

Mr. Walker.

Mr. E. O. WALKER: I have only to remark, in connection with what Sir David Salomons has just said, that a few years ago, on sending my earth-current curve to the Colaba Observatory at Bombay for comparison with the magnetic curve of the day, Mr. Chambers, who was in charge of the Observatory, noticed a distinct similarity in the rise and fall of the earth current with the change in temperature of the surface soil in Bombay, so that there must be a coincidence between the effect of temperature on the earth's surface and the earth current. It does not occur to me to say anything more on this subject just now. Observations in India are necessarily very limited, as we have few lines at our command that can be used for observation without detriment to traffic.

The President.

The PRESIDENT: In proposing a vote of thanks to Mr. Walker for bringing this subject before us, I think I ought to say, as it is a subject to which I have devoted a great deal of attention, and on which I have written several papers, that I have been intensely interested in the remarks that he has made. But I would point

The President.

out to him that I do not think he has indicated in any way that any direct distinction has been shown between the currents due to atmospheric causes and currents due to changes in the potential of the earth. Some 30 or 40 years ago there was an extremely able paper on this subject, giving the results of some observations made in America; at the present moment I really forget the author. I am not quite sure that it was not Professor Henry. However, it was shown in this particular paper unmistakably that it was necessary to draw a great distinction between earth currents and atmospheric currents, and by very ingenious contrivances it was shown that the currents due to atmospheric electricity were just as frequent as those due to changes in the potential of the earth. Mr. Walker also did let out one thing that has great interest. It is the suggestion that the specific inductive capacity of the air may change. We are always taught, and are always accustomed to regard the specific inductive capacity of the air as one of Nature's constants. It is generally assumed to have the value of 1. We do not know what the value really is, or what it ought to be; but for the purpose of calculation and mathematical reasoning it has been allotted the value 1. Now long experience of telegraphy has led me to believe that this is not true—that the specific capacity of air is not 1, and that the specific capacity of air is variable; and I have tried innumerable experiments in endeavouring to show that the presence of aqueous vapour in the air does change its specific capacity. There is this striking fact in all instances where I have compared the capacity of our wires in England with the capacity of those in other countries—that the capacity in England is invariably greater than that in other countries. That may be due to the fact that our atmosphere is more charged with humidity than it is certainly in America. It has been attributed, and I have attributed it myself, to the existence of earth wires on our poles. Although that may be one of the causes, the probability is that the change in the value of the specific capacity may also account for it.

Another important feature in Mr. Walker's paper is that he has given the value of these potential differences in volts. I have

tried, through the columns of the *Electrician*, to call attention to the fact that observations made on earth currents in different parts of the world are absolutely valueless to us at home because they do not give those very electrical measurements that enable us to draw comparisons. We want to know the voltage; we want to know the geographical direction of the lines observed; and we want to know their resistance. We can from those facts deduce all the electrical elements we desire. Mr. Walker has to some extent supplied this want, and he has given it to us in very clear language; and I am quite sure I am only echoing the sentiments of you all in proposing that we accord to him a hearty vote of thanks for his very interesting paper. The President.

The motion was carried with acclamation.

The PRESIDENT: I now call on Mr. Falkenstein to read his paper on "The Influence of Electricity in Tanning Operations."

NOTES ON THE INFLUENCE OF ELECTRICITY IN TANNING OPERATIONS.

By CONRAD K. FALKENSTEIN, Associate.

It has been known for the last thirty years that the application of an electric current to hides in process of tanning exerts a beneficial effect, increasing the rapidity with which the gelatinous material of the pelt combines with the tannin in the liquors to form leather. Before going further, it may be well to briefly sketch out what occurs when hides are tanned. Ox hide consists roughly of three layers, as follows:— Mr. Falkenstein

(*a.*) The *epidermis*, consisting of an outer layer, which is constantly being shed, and an inner layer, from which the material of the outer layer is supplied.

(*b.*) The *pars papillaris*, of close fibre, in and below which are situated the hair sheaths, fat and sweat glands, and the muscles which erect the hair; the upper coat of this layer forms the "grain" of leather.

(*c.*) The *pars reticularis* or corium, a network of fibres,

Mr. Falkenstein. which forms the greater part of the section; this is densest near the layer (*b*), the bundles of fibre becoming more open towards the flesh side. When the hide is taken off the animal the exterior of this layer is covered with flesh, fat, and tissue.

The first thing to be done is to wash the hide free from all blood, salt, &c., or, if sun dried or cured, to thoroughly wash and soften it (this is sometimes assisted by mechanical means); when clean and soft it is treated with solutions of lime of gradually increasing strength, which have the effect of swelling up the hide and splitting up or commencing to dissolve the fibres. This action takes place most vigorously at the two surfaces; on the grain side it dissolves away the outer cells or epidermis, and the hair *papilla* or knob of corium, which holds and supplies nourishment to the hair, thus allowing it to be easily scraped off by the blunt unhairing knife; on the flesh side it plumps up the *pars reticularis*, causing it to be easily distinguished (by its firmness) by the flesher from the loose membrane and fatty matter which is to be removed. When the hair and flesh are removed, the hides are washed and worked over on the grain side with a stone or blunt knife to force out the lime solution and the remains of hair, epidermis, &c., which are left in the hair cavities after unhairing; after another washing, the hides are ready for tanning.

Now the hide material or gelatine to be tanned consists of bundles of fibre, each bundle being made up of a refractory base, surrounded by gelatinous, tannable fibres, the axes of which lie generally at right angles to the surfaces of the hide. After liming, the fibres are in a swelled condition, both acids and alkalis (with exceptions) having this property of distending the hide and causing it to absorb water. The action of the tannic acid is to displace the water, and while converting the gelatinous fibres into leather, to maintain the plumpness by an acid reaction after the lime has been neutralised. Other methods are employed for unhairing on the Continent and elsewhere, such as sweating or partial putrefaction, use of sodium sulphide, &c, in which this plumpness has to be produced by the use of sulphuric, acetic, or other acids, or by liquors which contain acetic, lactic acids, &c., through natural fermentation.

Mr. Falkenstein

The hides are then treated with solutions of tannic acid of gradually increasing strength, until they show a uniform brown colour in a freshly cut section (lighter or darker according to the tanning materials employed), and have the necessary firmness and weight. Care must be taken as to the rate at which the tannic acid solution is increased in strength, for if, after the tannage is fairly commenced and the lime neutralised, the hides be not fed with stronger and stronger liquors, they lose their firmness, and are said to "go back," after which a species of putrefaction is set up in the interior untanned portion of the hide which causes adjacent bundles of fibre to unite, thus destroying the open fibrous structure of the hide, and practically stopping diffusion; this result may also be produced by using too strong liquors at first, which bind up the pores and prevent liquor from reaching to the interior.

For dressing leather somewhat different methods are followed, but the same precautions must be taken in working the liquors as just described.

The above is the process to be gone through, the functions of the electric current being—

1. To increase the rate at which liquor diffuses itself through the hide, by endosmose effects.
2. To increase the chemical activity of the reaction by molecular vibration.

Many methods of applying the current have been proposed, some depending upon the action of the gases evolved at the electrodes, and others upon the endosmose and molecular effects of the current on the hides and liquor. M. Gaulard patented a process in 1883, making use of the first method, the hides being suspended in a pit, from the perforated false bottom of which, is sent, first, a stream of hydrogen from the cathode; after eight days the current is reversed, and the hides subjected to the action of a stream of oxygen until they were tanned. The other method, which has been employed by Gaulard, Landin and Abom, De Meritens, and others, is to use a pit with electrodes on opposite sides, and pass the current through the liquor and suspended hides at right angles to their surfaces. When the hides are

Mr. Falkenstein.

struck through, they are transferred to the layer pits, where they are put in alternate layers with ground bark, &c., liquor being run in either before or afterwards.

The current is now applied by electrodes, placed one underneath and one on top of the pile, the positive pole generally at the bottom. This laying away is repeated until the hides are tanned. By thus applying electricity to the ordinary methods of the tannery, the time necessary for tanning heavy sole leather may be reduced from 120 to 45 days, as proved by the practice of Messrs. Landin and Abom in Sweden, and De Meritens at St. Petersburg. Messrs. Landin and Abom use alternating currents.

If now the electrical action be assisted by suitable mechanical agitation of the liquor and movement of the hides, so as to mix the liquor which has passed through the hide with that remaining in the vat, we shall obviously have an increase in the rate at which the tannage proceeds.

Several processes have been devised to combine the effects of the electric current and motion, amongst which may be mentioned those of Messrs. Worms & Balé, L. Groth, and others. Mr. Groth employs a vat, in the centre of which is a rotating or reciprocating (according to the shape of the vat) frame, on which are suspended the hides or skins to be tanned, an electric current being passed from one side of the pit to the other, through the liquor and hides. This process reduces the time necessary to tan a hide to about 28 days.

Messrs. Worms & Balé's system combines the application of an electric current with the use of a rotating drum; the drum is about 11 feet in diameter, and provided with internally projecting pegs; in operation it is nearly half filled with liquor, and the hides being put in (one by one), the drum is closed and rotated; while in motion an electric current is applied by electrodes, so disposed as to fill the liquor with, and hence subject the hides to, a uniform field of endosmotic force. When the drum is in motion, the hides, floating in the body of the liquor, are turned over, and the liquor mixed, the bottom layers being continually brought to the top by the skin friction. Under these conditions,

running continuously, hides may be tanned in about 100 hours. Mr. Falkenstein.
It will be seen that the rotating drum contains four variable factors—

1. The strength and disposition of the electric current.
2. The speed of rotation
3. The proportional amount of liquor in the drum.
4. The length and number of the projecting pegs.

Thus giving the system considerable flexibility of treatment. It has now been worked on a commercial scale for some years by the British Tanning Company, Limited, in London, by M. M. Brion et Dupré, in Paris, and also in America.

Considering the purely electrical and chemical effects on the hides and liquor, the final result has been established by the experiments of Messrs. Rideal and Trotter, Professor Müntz of Paris, Dr. Foelsing, Dr. Zerener, and others—viz., raw hide is capable of combining with its equivalent weight of tannic acid in from 4 % to 38 % (according to the process employed) of the time taken when electricity and apparatus, or electricity alone, is not applied.

Below I give particulars of some experiments which I have made, bearing on the action of the electric current. Let us first consider what happens when a current is passed through a carefully filtered solution of tannic acid, made from an oakwood extract, using platinum electrodes. The cell employed was divided into three by two porous clay diaphragms, each compartment being of equal capacity, 200 c.c., and containing 150 c.c. In the first compartment was placed the anode, and in the third the cathode, the intermediate one completing the circuit. A current of 0·0024 ampere was applied at a P.D. of 7 volts. On completing the circuit the following actions took place: At the anode bubbles appeared and collected round the wire as a straw-coloured froth; at the cathode there was considerably more evolution of gas, and the froth produced was darker in colour and showed streaks of dark-red coloration radiating from the wire. After passing the current for 90 hours it was stopped, and the electrodes and liquor examined. The anode had a small quantity of soft, brown precipitate on its surface ($\frac{1}{8}'' \times \frac{1}{8}''$) which was easily soluble in

Mr. Falkenstein. warm, distilled water, and was absorbed by hide powder; the cathode was covered closely with a hard, dark-brown coat of precipitate to the depth of from 1-64th″ to 1-32nd″ at the edges. This precipitate is insoluble in hot distilled water, but is immediately dissolved on addition of a very small quantity of H_2SO_4, forming a clear tan-coloured solution which is bleached on treatment with hide powder, thus proving the precipitate to be a dark, tanning colouring matter.

On examining the liquid in the three compartments, No. 1, containing the anode, was as clear as at the commencement of the experiment, so was No. 2, both having a slight coat of tannin anhydride on the bottom; No. 3, containing the cathode, was somewhat turbid with dark-brown particles which were soluble in hot distilled water and absorbed by hide powder.

At the end of the experiment it was found that in the compartments 1 and 2 the level of the liquid had fallen and in 3 it had risen.

The above gives a general view of the action, which I have amplified as follows:—

I. To compare the endosmose effects in solutions of tannic acid, at varying potential differences, at varying strengths of tannic acid, and with diaphragms of different materials.

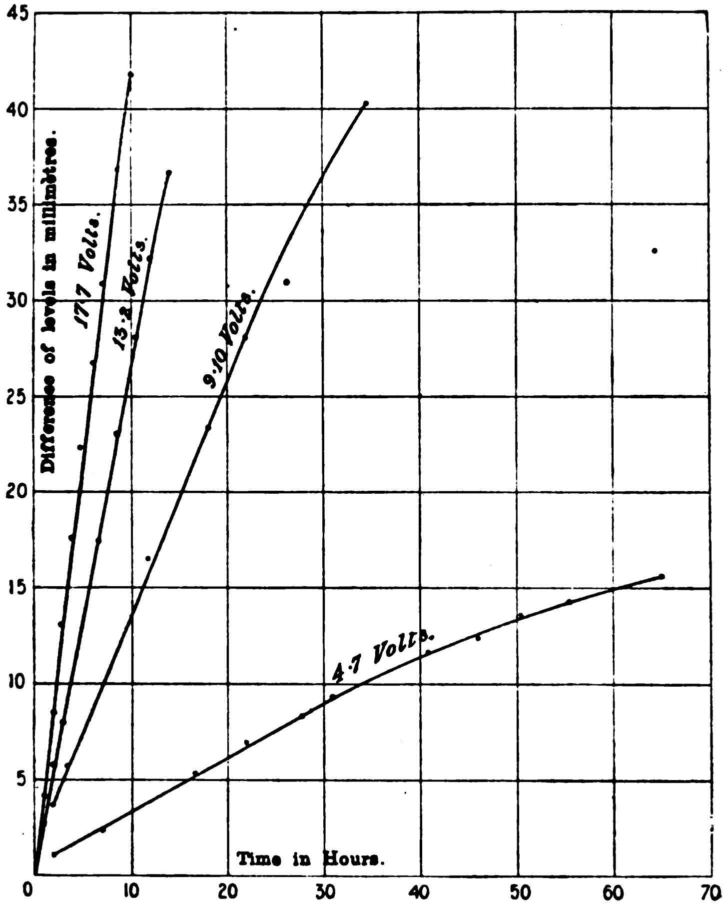
II. To compare the precipitation of anhydride, &c., in solutions of tannic acid, when a current of electricity is passed through them and when allowed to remain during the same time and under the same conditions, but without passing a current.

III. To examine the amount of tannin in the liquor before and after passing an electric current.

I.—Endosmose.

Cells were made consisting of two ebonite boxes (each having one side cut away) fastened together with a plate of porous clay or leather between them, thus forming two compartments, joined through a porous diaphragm, each having a capacity of 200 c.c. In experimenting, 100 c.c. of the liquor to be examined was placed in each compartment, and the current applied from accumulators, &c., through platinum electrodes, and the variation

of level with time noted; the top and bottom surfaces of the cells were made parallel and carefully levelled during experiments. Between each set of readings the clay diaphragm was cleaned and allowed to soak in water, so that the mechanical resistance to flow of liquor might remain as nearly constant as possible. Mr. Falkenstein.



CURVES A_1.—1 % Solution of Tannin.

This method was adopted as the apparatus is simple, and if differences of level between the two sides of the diaphragm be considered, evaporation errors are eliminated, as the surfaces exposed to the air are equal in both compartments.

The first experiments undertaken were to see in what way

Mr. Falkenstein. the rate of flow varied with the difference of potentials applied, the results of which are shown in the accompanying curves.

CURVES AND TABLE A_1 AND A_2.

I.	II.	III.	IV.	V.	VI.	VII.
Volts.	(Volts)^2.	Tangent to Curve.	Mean Current by Voltameter.	Proportional Rate of Flow.	Calculated Rate of Flow.	Proportional Current.
4·7	22·09	0·27	0·00089 A	0·22	0·27	0·13
9·0	81·00	1·22	0·0066 „	1·00	1·00	1·00
13·2	174·24	2·71	0·0162 „	2·22	2·15	2·45
17·7	313·29	4·42	0·0274 „	3·62	3·86	4·15

From A it may be seen that—

(1) With an approximately constant mechanical and electrical (at one voltage) resistance, the rate of flow of the liquor varies at a higher rate than simple proportionality to the voltage between electrodes (see columns I. and V.).

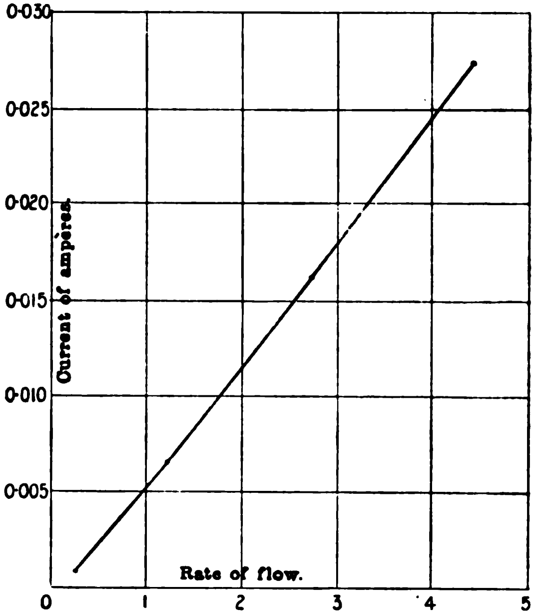
(2) The current varies at a slightly higher rate than the rate of flow, as measured by tangent to curve (see columns V. and VII.; see also curve A_2).

(3) The rate of flow approximately varies as the square of the voltage (see columns V. and VI.).

In order that Ohm's law may be satisfied, it follows that the resistance of the tan liquor examined must vary inversely as the potential difference applied. In some experiments I am making on this point, I find that between certain limits of current-density the resistance varies inversely as the electro-motive force, but after the solution has been subjected to electrolysis for some time at the higher current-density it loses its power of offering an increased resistance to a lower potential difference, the resistance remaining fairly constant at the minimum value for all potential differences below the high one which broke it down.

This, taken in conjunction with the results given in Division III., points to an alteration in the structure of the tannin molecule

being produced by electrolysis, the resinous dark tanning matter being separated in the form of insoluble precipitate on and about Mr. Falkenstein.



CURVE A_2.

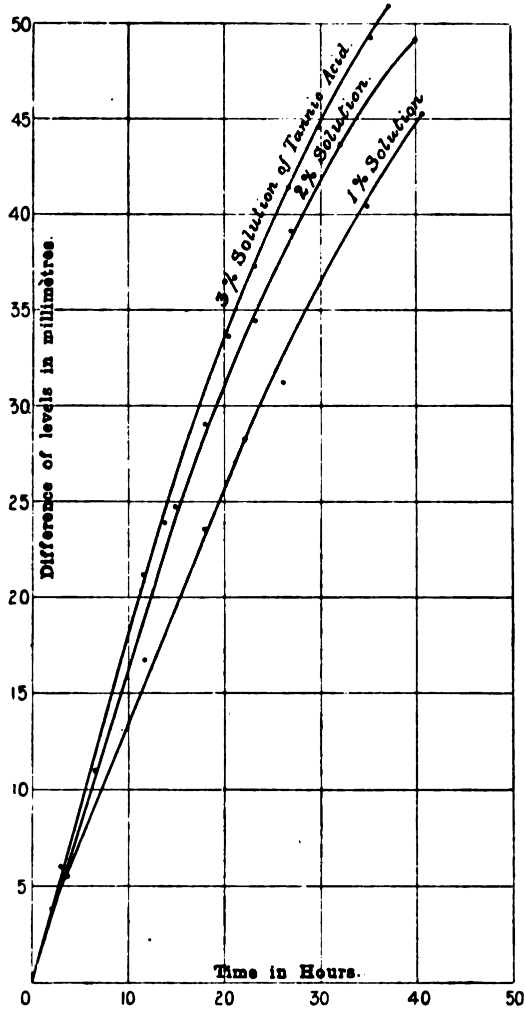
the cathode, the remaining liquor being of higher electrical conductivity and clearer, the amount of available tannin remaining constant, although in separate forms. The next experiments were made to find in what way the rate of flow varied with the percentage of tannin in the solution, at constant potential difference, and using the same porous diaphragm in each case.

Table B.—CURVES B_1 AND B_2.

Percentage of Tannin.	Volts.	Mean Current.	Rate of Flow.	Proportional Rate of Flow.	Proportional Current.
1	9·0	0·0066 Amp.	1·22	1·00	1·00
2	9·0	0·0079 „	1·60	1·31	1·19
3	9·0	0·0109 „	1·80	1·47	1·65

From the above it will be seen that the rate of flow (as

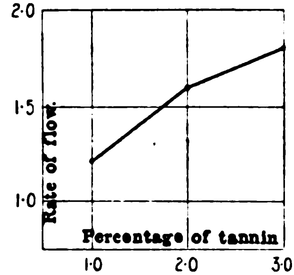
Mr. Falkenstein measured by tangent to curve) increases with the strength of the liquor.



Curves B_1.—Potential difference = 9·0 volts.

This fact is specially important, for the following reason: In ordinary tanning operations it is well known that the greater portion of the gelatine is combined in the first month or so of the tannage, the remainder of the time being spent in tanning the middle layers and giving weight, owing to the increased resist-

ance which the outer tanned portions offer to diffusion. Now, for reasons given earlier in the paper, it is necessary to use liquors of gradually increasing strength as the tannage proceeds, and which, for any given potential difference, give (as shown in curve B_2) an increasing hydrostatic pressure or head tending to force the liquor through the hide; thus there is a kind of self-regulating arrangement—viz., as the mechanical resistance of the hide increases, so the endosmotic pressure increases to overcome that resistance. Mr. Falkenstein.



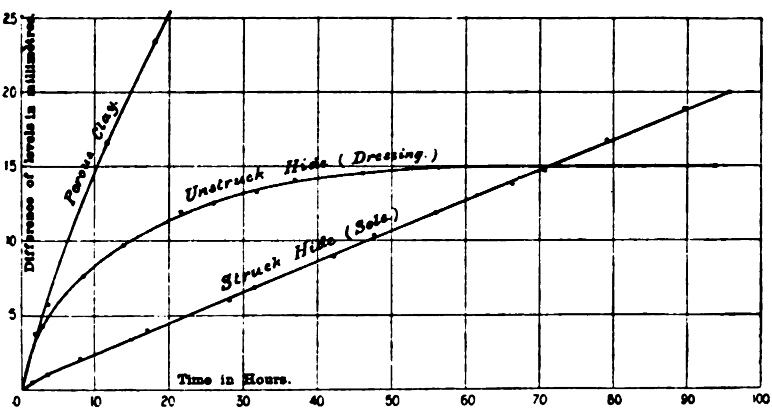
CURVE B_2.

The next experiments were made, using diaphragms of different materials and keeping potential difference and strength of liquor constant, the results being shown below:—

Table C.—CURVES C.

9 volts. 1 per cent. Solution of Oakwood Tannin.

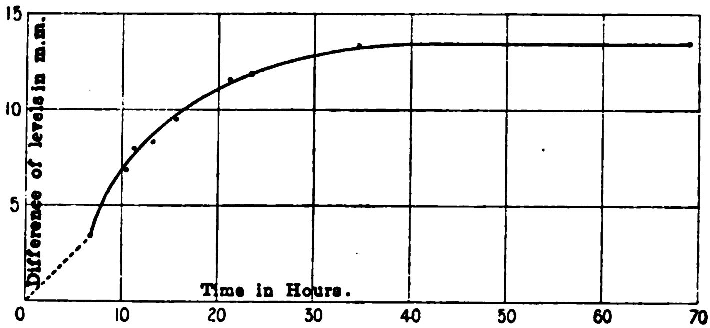
Diaphragm.	Mean Current.	Rate of Flow.	Proportional Current.	Proportional Flow.
Struck sole ..	0·0041 *a*	0·20	0·62	0·16
Porous clay ..	0·0066 „	1·22	1·00	1·00
Dressing hide	0·0106 „	1·58	1·60	1·29



CURVES C.—1 % Solution of Tannic Acid. P.D. = 9·0 volts.

Mr. Falkenstein.

In the above will be noticed the great difference in mechanical resistance to diffusion between leather, before and after it has been struck (*i.e.*, smoothed down and worked over with the striking pin) and rolled. The curious bending over of the curve for unstruck hide is due to the influence of the head of liquor and openness of the pores allowing the liquor to run back again, a certain point being reached at which this leakage becomes equal to the flow, and the difference of levels remains constant. This effect is also shown in Curve D with tap water and a porous clay diaphragm.



CURVE D.—Porous Clay Diaphragm (Tap Water). P.D. = 7·2 volts.

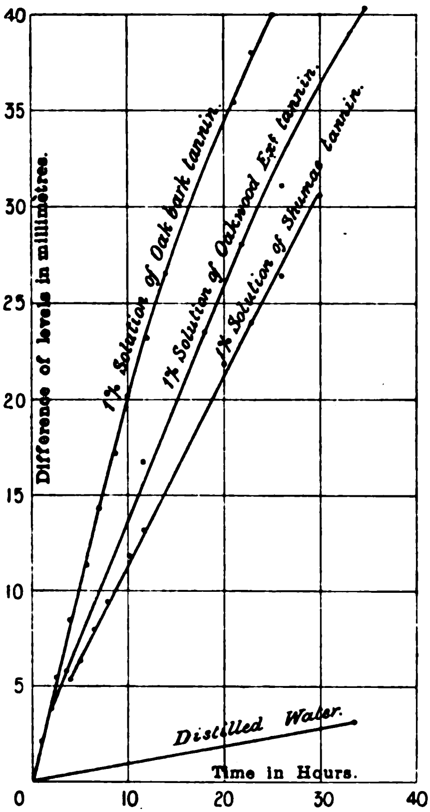
The next experiments were made, using liquors prepared from different tanning materials, and keeping the potential difference constant and using the same porous clay diaphragm, the results being shown below :—

Table E.—CURVES E.

9 volts. 1 per cent. Solutions.

Liquor.	Mean Current.	Rate of Flow.	Proportional Current.	Proportional Flow.
Distilled water..	0·00078	0·095	0·12	0·077
Shumac	0·0204	0·97	3·09	0·79
Oakwood extract	0·0066	1·22	1·00	1·00
Oak bark	0·0164	2·02	2·48	1·65

In Curves E 10 mm. difference of levels corresponds to 11·36 c.c. passing through the diaphragm, the levels in both compartments being the same on starting experiment. Mr. Falkenstein.



Curves E.

II. and III.—Chemical Action.

II. To compare the precipitation of anhydride, &c., in solutions of tannic acid, when a current is passed through them, and when allowed to remain during the same time and under the same conditions, but without passing a current.

III. To determine the amount of tannin in the liquor before and after passing an electric current.

In carrying out the above, two beakers were taken, in each of which 150 c.c. of tan liquor (containing 5 per cent. oakwood tannin)

Mr. Falkenstein.

was placed. Through the liquor in one beaker a current of 0·0086 ampere (measured by voltameter) was passed for $96\frac{1}{6}$ hours, and the other was allowed to stand open by its side, being thus under exactly the same conditions, excepting that it had no electricity passed through it.

No.	Description of Liquor.	10 c.c. Infusion.	10 c.c., after Hide Powder.	c.c. of Non-Tannin.	Tannin %.	Non-Tannin %.
I.	Liquor before experiment	32·75	19·5	1·9	5·007	0·69
		32·70	19·4	1·8		
II.	After exposure, without passing electric current ...	32·50	19·55	1·95	4·88	0·72
		32·45	19·50	1·9		
III.	No. II., filtered ...	32·15	19·45	1·85	4·79	0·69
		32·15	19·45	1·85		
IV.	After passage of electric current, and exposure ...	32·5	19·0	1·4	5·073	0·54
		32·5	19·1	1·5		
V.	No. IV., filtered ...	31·0	18·9	1·3	4·57	0·48
		31·0	18·85	1·25		

In the above analyses,

33·4 c.c. of permanganate = 10 c.c. $\frac{n}{10}$ Crystallized Oxalic Acid.

17·6 " " = 20 c.c. Indigo Carmine.

The same solutions were used throughout the above experiments; 10 c.c. of a diluted solution (1-20th) were titrated with permanganate in presence of indigo carmine before and after treatment with hide powder. On examining the above results we may notice—

(i.) The action of the electric current on bodies which are absorbable by hide powder.

(ii.) The action of the electric current on bodies which are not absorbable by hide powder. Mr. Falkenstein.

(i.) On comparing Nos. I. and IV. it will be seen that the total amount of tannin remains unaltered, or, if anything, to have slightly increased after the passage of the current. On filtering IV. and again analysing, it will be seen that some of the tannin has been precipitated (see V.), probably as anhydride or phlobaphene. This precipitate, as seen from the experiment with the three-compartment cell, is produced at the cathode by the action of the hydrogen evolved there; this tannin is proved by column IV. to be still available for tanning purposes (after heating).

(ii.) This action is very marked, as will be seen from examination of columns 5 and 7 in table: the number of c.c. of permanganate consumed by these non-tannin bodies goes down from 3·7 in the original liquor to 2·9 in the unfiltered electrolysed solution, and to 2·55 in the filtered; this reduction is really greater than is apparent here, because the electrolysed solution was exposed to the air for exactly the same time as the other, and therefore the increase of 0·15 c.c. produced by exposure in the non-electrolysed should be added. This leads one to think that there may be a kind of restoration of the liquor, the tannin which has been decomposed by exposure to the air being reduced, possibly by the nascent hydrogen at the cathode: this would account for the reduction in the quantity of non-tannin matters, although, if these were more easily oxidisable than the tannin, the oxygen evolved at the anode might influence this result.

Summary.—The principal chemical actions of the current on tan liquor, using platinum electrodes at a current density of 1·6 amperes per square foot, are—

1. Precipitation of anhydride of tannin; this precipitate is more copious with a tannin capable of yielding "bloom," as shumac, in which case the electrical conductivity is higher (see Table E).

2. Reduction of the amount of non-tannin matters in the solution. The above shows that, at all events below current-densities of 1·6 amperes per square foot, a limit which is never exceeded or reached in practice, there is no loss of tannin

Mr. Falkenstein. due to the action of the electric current, as the ratio $\left(\frac{\text{number of ampere-hours}}{\text{number of c.c. of liquor}}\right)$ in this experiment is considerably greater than that employed in practice—say 366 : 1. I have found that the solubility of the precipitate decreases as the current-density increases.

There is an effect (referred to earlier in the paper) which doubtless influences the rapidity at which the gelatine and tannin combine—viz., the molecular vibration of the liquid and hides, due to the phenomenon of electricity being a vibration of the ether, agitating the particles and making them combine with their affinities sooner than they would do if left to themselves. In support of this, the fact of tanning operations being accelerated by rise of temperature may be brought forward, heat being a similar vibration of the ether, differing only in period from the electrical vibration.

In endosmose, however, we have a perfectly tangible, measurable effect, propelling the liquor in the direction of the lines of current-flow, tan liquor also being a particularly favourable medium for this purpose, for the following reasons:—

1. The force increases with the percentage of tannic acid in the solution, thus falling in with the practical methods of tanning—*i.e.*, increasing as the permeability decreases.

2. The electrical resistance of tan liquor being high, the necessary difference of potentials (to the square of which the endosmotic flow is proportional) can be obtained between adjacent portions of the liquid without an undue expenditure of current, and therefore of energy.

We might approximately calculate how much tannin would be likely to pass through a 20-lb. butt (tanned and dry wt.) in an hour, using, say, a 12-ampere current at 9 volts, with proportionally larger electrodes, but keeping the distance between electrodes and hide the same as in experiment C with unstruck hide (it must be remembered that the leather in question was fully tanned, and therefore its permeability was at its minimum value).

Area of butt = 54″ × 66″ = 3,564 square inches.

Working area of diaphragm = $2\frac{5}{8}$ square inches. Mr. Falkenstein.

Tangent to curve near origin = $\frac{10 \text{ mm.}}{6{\cdot}3 \text{ hours}} = \frac{11{\cdot}36}{6{\cdot}3}$ c.c. per hour.

Current in experiment = 0·0106 ampere.

One gallon = 4,536 c.c.

Increasing the current to 12 amperes, and assuming (from Table A and Curve A_2) that the flow is proportional to the current, we have $\frac{11{\cdot}36 \times 12}{6{\cdot}3 \times 0{\cdot}0106 \times 4{,}536} = 0{\cdot}45$ gallon of liquor passing through the butt per hour; this, supposing the liquor to contain 4 per cent. of tannin, and to have a specific gravity of 1·02, would be equivalent to $\frac{0{\cdot}45 \times 10 \times 1{\cdot}02 \times 4}{100} = 0{\cdot}1836$ lbs. of tannin. If the butt require $20 \times 0{\cdot}45 = 9$ lbs. tannin to tan it, then this amount would pass through in 49 hours. From the above it will be seen that, having a means of bringing the liquor into intimate contact with the fibres of the hide, and also of assisting it to combine whilst passing through, we are in a much more favourable position than when working with only diffusion and capillary attraction to bring the liquor to the interior and replenish it when exhausted, especially as the tannage proceeds and the hide becomes less and less permeable.

In conclusion, I must express my thanks to the British Tanning Company, Limited, for facilities granted in carrying out experiments, and permission to publish results.

Mr. Swinburne. Mr. SWINBURNE: The only remark I have to make is in the way of congratulation to Mr. Falkenstein. The paper seems to me to be full of most important information, and it is a very valuable paper indeed. One can hardly discuss it without having seen it before.

The President. The PRESIDENT: I think, at this late hour, and considering the fact that we have not had the paper before us to study, we will avoid discussion, and simply propose that we accord to Mr. Falkenstein a vote of thanks for his very able paper. I think myself it is a great advantage to this Institution that we have from time to time brought before it new and fresh subjects. There are many members of this Institution who seem to labour

The President. under the impression that the whole electrical world is to be found in the magnetic field of a dynamo. It is just as well sometimes that they should be reminded that there are other people in this world who are studying other forms of electrical agency and electrical action. We have to thank Mr. Falkenstein for the very great care he has taken in bringing this paper before us.

I have to report that the scrutineers declare that the following candidates have been duly elected:—

Member:

Reid, Edwin J.

Associates:

Coates, Herbert John. | Durtnall, William.

Students:

Garrick, John Hayton. | McDonough, Alfred.
Day, P. J. C. | Smith, F. Wood.
Gamlen, Robt. Loraine. | Soloman, H. G.
Spence, John Outhwaite.

The meeting then adjourned.

				1,275	10	0
Salomon's Scholarship Fund—						
£1,000 0 0 New South Wales $2\frac{1}{2}$ % Stock, cost				1,087	10	6
Furniture—						
As per last Balance-Sheet ...	£243	11	4			
Less depreciation, say	24	7	2			
				219	4	2
Overdue Subscriptions estimated to realise				162	17	0
Stock in hand of Institution's Journals and Ronalds' Catalogue, estimated value				651	18	4
Books, Pictures, &c. (other than the Ronalds' Library),						
As per last Statement	£887	0	0			
Add value of Books, &c., since purchased and presented ...	50	0	0			
				937	0	0
Amount due for Advertisements in the Journal ...				72	0	0
Amount due from the Technical Press for Reports of Discussions				7	15	0
Cash at Bankers, as shown above				£839	2	5
				£7,871	15	8

£7,871 15 8

We certify that we have examined the Books, Vouchers, and Securities of the Institution, and that the above Statements of Receipts and Expenditure and of Assets and Liabilities are correct, and exhibit the true financial state and condition of the Institution.—

WAGSTAFF BLUNDELL, BIGGS, & CO., Chartered Accountants,
12, Delahay Street, S.W.

March 17, 1898.

FRED. CHAS. DANVERS, } *Honorary*
AUGUSTUS STROH, } *Auditors.*

ORIGINAL COMMUNICATION.

REPAIRS TO SUBMARINE TELEGRAPH CABLES.

By F. A. Hamilton, Member.

Electrical engineers and others have from time to time derided the Lords of the Admiralty of a bygone day* for their tenacity in clinging to the primitive method of signalling by means of the semaphore, which their Lordships pronounced to be quite equal to the needs of the service.

They who reside in edifices constructed of a frangible and transparent material should abstain from hurling petrified reproach, for the same spirit which influenced the rulers of Her Majesty's Navy still holds sway among some who are apt to cherish a feeling of thankfulness that they are not "even as these "Admirals."

An instance of the singular but apparently general characteristic above mentioned is presented in the case of a really remarkable invention submitted to the Society of Telegraph Engineers† more than eight years ago by H. Kingsford, Member, a full account of which will be found, with the discussion that followed, in vol. xii., page 490, *et seq.*, of the Society's Proceedings, under the title of "Trott and Kingsford's Automatic Grapnel for "Submarine Cables and Torpedo Lines." The grapnel there described has proved invaluable in many important cable repairs, as will be seen on referring to the appendix to this paper.

The grapnel contains an insulated wire—a continuation of the heart of the grapnel rope—connected to an insulated metal disc recessed in the crown of the grapnel. Pushes, which in their normal position are caused to project—by means of spiral springs

* About that time—1842—a proposition was made to the Admiralty for running a wire to Portsmouth, but so little was the new discovery relied on that their Lordships decided upon having no other telegraph than the chain of semaphores then established.—Captain H. A. Moriarty, R.N., C.B., *Journal Royal United Service Institution*, vol. xi., page 127.

† Now called "The Institution of Electrical Engineers."

of any desired strength—are inserted between the shank and prongs. These pushes are fitted with fine steel points, which, when driven in by pressure being applied to the head of the push, make contact with the metal disc, and consequently bring into action the battery and bell connected with the conductor contained in the grapnel.

Such an apparatus *must* work; there is positively nothing with regard to it either in principle or in practice to which reasonable objection can be made.

It is true that this grapnel, like any other, can be broken in unskilful hands; but as it is to be hoped that such a condition will be the exception, and not the rule, this consideration need not occupy our attention.

The only weighty argument one can imagine as likely to flash across the minds of some, is the contention that this grapnel will reduce the art of hooking cables to one of such extreme simplicity that there will be *nothing in it.* But it must be remembered that raising the cable will always require a certain amount of skill—measured by the condition of the cable—and any appliance tending to enhance the success of the operation is worthy of the attention of those who have to attempt this frequently difficult task; for it will be readily admitted that the sooner the fact of the cable having been hooked is known, the greater the likelihood of its being raised to the surface, it being evident that dragging the cable over the bottom, especially on rough ground, is not calculated to increase the chances of its being lifted; and, as it is very important that the exact line of the cable be ascertained, it is manifestly an advantage to have a grapnel that will indicate the moment the cable is touched, for with the ordinary method of grappling it frequently happens that the cable is hooked and broken without the slightest suspicion of the fact being known on board the ship.

The satisfactory results obtained with the automatic grapnel here mentioned, justify the statement that it has been the means of effecting not only a very considerable saving both of time and material, but of actually restoring cables that would, if subjected to the old treatment, in all probability have been abandoned.

It must be admitted that the ordinary practice of relying on the dynamometer as an instrument for indicating when the cable is hooked is extremely unsatisfactory. This statement is fully borne out in reports of operations in deep water, the entry, "Cable "probably hooked," being the usual formula when the grapnel is supposed to have crossed the line of cable.

These records will also show that the grapnel has frequently to be raised some considerable distance from the bottom before the cable engineer can be absolutely certain whether the cable is hooked or not. It is also obvious that the cable must be dragged out of line before the dynamometer begins to indicate any abnormal strain.

The automatic grapnel circuit is now as reliable as that of any ordinary electric bell, and there can be no doubt that, once having experienced the comfort of working with it, especially in deep water, the cable engineer will for ever discard the dynamometer as an instrument for indicating the presence of the cable on the grapnel.

There can be no question that this simple apparatus, which has been the means of resuscitating some of the oldest cables in the Atlantic, will revolutionise cable-repairing operations generally.

The cable engineer need no longer have recourse to the uncomfortable expedient of sitting on the grapnel rope in the frequently vain endeavour to ascertain whether the cable is hooked.

Had the expeditions which were repeatedly fitted out at prodigious cost for the attempts to repair the 1865 and 1866 Atlantic cables, been armed with such grapnels as the one here described, there is little doubt that property, representing about two millions sterling, now lying abandoned in the North Atlantic, would to-day be earning dividends; and, further, that were it not for the fact of other cables having been laid in proximity to the tracks of the old ones, *the latter could even now be recovered* and repaired by means of the automatic grapnel.

The following typical cases will serve to show the advantages of the method of grappling now presented for consideration :—

1. *September 9th,* 1887.—North Atlantic, southern edge of Grand Bank. Lowered an ordinary grapnel in 1,096 fathoms, and towed until 4 a.m. of the 10th, when, having reached what was considered a safe limit, having regard to other cables in the vicinity, began heaving in. The grapnel was being hove up at a rapid rate, there being no suspicion of the cable being hooked, when the strain shown on the dynamometer dropped suddenly from 2½ tons to ¾ ton, the grapnel then being 10 fathoms from the surface. On heaving it to the bows, a short piece of cable was found on the prongs. Had the automatic grapnel been used on this occasion, such an accident as towing through the cable and raising it nearly to the surface without its presence being known would probably not have occurred, and the repair consequently have been effected on the 10th instead of on the 12th.

The foregoing negative proof of the value of the automatic grapnel may be aptly followed by a positive one.

2. *July 27th,* 1888.—1.50 p.m.: Began grappling towards line of cable. This position—longitude 51° W.—is in close proximity to the numerous cables that crowd the southern extremity of the Grand Bank. Dragged until 8.58 p.m., when, having towed as far as was considered prudent under the circumstances, began heaving in the grapnel rope. The rope was being hove in at a rapid rate when the bell suddenly rang, and the deflection on the indicator in circuit with the grapnel showed one of the pushes to be fully depressed. This timely warning prevented the inevitable rupture of the cable, for there was no suspicion of its being hooked. Heaving in the rope was proceeded with gently, the cable was raised to the surface, and the repair completed at nine o'clock the next morning. A continuance of bad weather after this date emphasised the value of the grapnel.

3. *Repair of the* 1869 *Cable in Mid-Atlantic.*—The ship left England on May 19th, 1888, and on the 23rd a mark buoy was placed on the line of cable in about longitude 27° W., near the position of a splice made by the "Scotia," during a former repair, in September, 1882. On the 30th, whilst making the sixth tow, we were cheered by the ringing of the bell in circuit with the

grapnel. The presence of the cable would not have been known had we been using an ordinary grapnel, for, although the above-mentioned indication continued for nearly an hour, the strain shown on the dynamometer did not rise above the normal. The cable was raised 75 fathoms, when the deflection on the instrument in circuit with the grapnel suddenly fell to zero, showing that the cable had broken. The following day brought a like occurrence, plus the fact of a short piece of the cable, including the aforementioned "Scotia's" splice, being recovered. The grapnel on this occasion signalled, "Cable hooked," long before there was any indication on the dynamometer. Here again it may be truly stated that but for the automatic grapnel we should have "gone through" the cable without knowing we had touched it. On the 4th, we hove up two short ends, the bottom in this vicinity being extremely rough, and the cable consequently much deteriorated. Operations on the eastern side of the fracture were begun on June 14th, in 1,900 fathoms, and after we had towed for five hours, the bell once more began to ring, not with the feeble tinkling which is occasionally heard when stones rap against the pushes, but with a decisive, sudden, and continuous peal, indicative of the plunger or push being fully depressed. Not until *thirty-six minutes afterwards* did the dynamometer begin to show a rising strain, by which time the cable had been lifted 418 fathoms from the bottom.

4. *June 8th*, 1889.—8.45 a.m.: Lowered grapnel in 33 fathoms. 11.30 a.m.: Bell ringing; good contact made for about 20 seconds. Hove up, and found about 8 fathoms of the abandoned 1867 cable on the grapnel. It is needless to say that the dyna mometer failed to indicate the presence of this relic.

5. *Repair of the* 1869 *Brest-St. Pierre Cable.—August* 12*th*, 1889.—Longitude 30° 45′ W.; depth, 1,800 fathoms. 4.50 a.m.: Began lowering grapnel; 2,610 fathoms of rope in circuit. Resistance of conductor = 33 ohms; resistance of insulator = 5,000 ohms; towing strain, 3½ to 4 tons. 12.52 p.m.: Slight ring. 1.3 p.m.: Bell ringing steadily; signalled to the bows, "Cable "hooked;" depth, 1,850 fathoms. 4.7 p.m.: Buoyed bight of cable 1,365 fathoms from the surface.

August 13*th.*—Towed a cutting grapnel through the cable, then took the "bight buoy" on board and hove in on grapnel rope; deflection on indicator, 75°. 4.4 p.m.: Cable at the bows; O.K. to St. Pierre.

During this repair 11 tows were made, the grapnel never once failing to indicate the moment the cable was hooked.

6. *Repair of Brest-St. Pierre* (1869) *Cable.—December* 10*th*, 1889.—11.43 a.m.: Lowered grapnel; depth, 84 fathoms. Towed until 6.55 p.m., when hove up and found a short piece of the cable on grapnel, which, owing to the "rounding" placed over the coupling having slipped down the shank, had failed to indicate.

This unfortunate accident tends to show how important it is that this grapnel should be kept in a thorough state of efficiency, and, further, that an ordinary grapnel is of little use in operations on old cables.

Had the grapnel not failed on this occasion, the repair might have been finished the same day, instead of two days later, and the ship would have escaped a heavy gale.

Only on rare occasions, and chiefly in shallow water, has any other kind of grapnel been used during the last six and a half years in repairs to the Anglo-American Telegraph Company's cables. Electrically the grappling apparatus used in these repairs may be pronounced almost perfect, but mechanically it is still slightly defective, owing chiefly to the lower end of the shank being too full and likely to prevent the cable from "taking" fairly on the pushes. This defect has been pointed out to the makers by the writer, and will doubtless be remedied. Notwithstanding this, and other imperfections, it may be claimed that the grapnel has proved itself an extremely valuable money-saving instrument, and certainly one that submarine cable engineers will in time appreciate.

The advantages of the method of grappling here mentioned are manifest and manifold, as shown in the appendix, the mere perusal of which will convince the reader that the automatic grapnel has not only proved itself of immense value in the past, but that it has also increased the value of submarine cable

property generally, inasmuch as it has been the means of restoring to life cables which would, in all probability, have been abandoned.

APPENDIX.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1885.		No. 1.—No. 1 *Automatic Grapnel.*
1	July 27	960	Bell rang. Cable hooked.
2	„ 29	1,373	Bell rang. Cable hooked. Bell suddenly ceased ringing. Cable parted 771 fathoms from the surface.
3	„ 29	1,580	Unsuccessful tow. Strain rose, but bell silent. Hove grapnel off bottom, when strain fell.
4	„ 30	1,636	Bell rang. Cable hooked, but afterwards slipped over grapnel, when bell ceased ringing.
5	„ 31	1,666	Bell rang. Cable hooked. Cable slipped over grapnel. Bell ceased ringing.
6	Aug. 3	1,146	Bell rang. Cable hooked. Cable slipped over grapnel. Bell ceased ringing.
7	„ 4	1,083	Bell rang, and continued to ring. Hove up, and found one prong of grapnel broken. Section of fracture honeycombed.
			No. 2.—No. 2 *Grapnel.*
8	Sept. 4	70	Bell rang. Cable hooked.
9	„ 4	53	„ „
10	„ 5	53	„ „
			No. 3.
11	Sept. 7	37	Cable hooked. Grapnel failed to indicate, owing to contact plate not being properly adjusted.
12	„ 7	37	Bell rang. Cable hooked. Repair completed.
			No. 2 (*continued*).
13	Sept. 8	53	Unsuccessful.
14	„ 8	53	Bell rang. Cable hooked. Repair completed.
			No. 4.
15	Sept. 10	40	Cable hooked. Grapnel failed to indicate. Contact plate not properly adjusted.
			No. 5.
16	Sept. 30	82	Unsuccessful.
17	„ 30	82	„
18	Oct. 1	89	„
19	„ 3	89	„
20	„ 3	89	Cable hooked. Grapnel failed to indicate being choked with stiff clay.
21	„ 4	156	Unsuccessful.
22	„ 4	162	Bell rang. Cable hooked.

No. of Tow.	Date.	Depth in Fathoms	Remarks.
	1885.		No. 6.
23	Oct. 13	79	Bell ringing slightly at intervals, indicating stony ground. Unsuccessful tow.
24	" 13	79	Unsuccessful.
25	" 13	79	Bell rang. Cable hooked.
26	" 13	96	Unsuccessful. Foul rope.
27	" 13	96	Bell rang. Cable hooked.
28	" 13	96	" "
29	" 13	97	" "
			No. 7.
30	Nov. 7	70	New contact plate inserted. Tow unsuccessful.
31	" 8	70	Unsuccessful. Bell ringing occasionally for a few seconds. Stones rapping against the pushes. Bell rang steadily. Signalled to bows, "Cable hooked." Hove up, and found rope foul of grapnel.
32	" 8	70	Unsuccessful.
33	" 8	70	"
34	" 8	80	Unsuccessful. Bell ringing intermittently and frequently for a few seconds, indicating the stony nature of the bottom. Hove up grapnel. Found the core squeezed close to shank of grapnel. Damage repaired. Owing to the extremely rough nature of the bottom, a short-pronged ordinary grapnel was used in completing this repair.
	1886.		No. 8.
35	Feb. 17	65	Bell rang. Cable hooked.
36	" 17	65	" "
			No. 9.
37	Sept. 6	61	Bell rang. Cable hooked.
38	" 6	61	Unsuccessful. Bell rang. Strain rising. Hove in on rope, when strain fell and bell ceased ringing.
39	" 6	61	Bell rang. Cable hooked.
			No. 10.
40	Sept. 8	68	Unsuccessful.
41	" 8	100	"
42	" 8	67	"
43	" 8	60	Cable hooked. Grapnel failed to indicate. Fittings had been taken out and cleaned, but hurriedly replaced.
44	" 8	60	Unsuccessful.
45	" 13	60	One prong of grapnel broken short off at crown.
			No. 11.
	Dec.	—	Received two new grapnels.
	1887.		
46	Mar. 9	62	Bell rang. Cable hooked. (*No. 3 Grapnel.*)
47	" 10	62	" "
48	" 14	62	Unsuccessful.
49	" 14	62	"
50	" 14	62	"
51	" 14	62	"
			The bottom being extremely rough, substituted an ordinary grapnel during this repair.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1887.		No. 12.
52	Apl. 21	86	Bell rang. Cable hooked.
53	„ 21	86	Unsuccessful.
54	„ 21	86	„
55	„ 21	86	Indicator deflected slightly. Cable hooked.
			No. 13.
56	July 26	2,097	Unsuccessful.
57	„ 27	2,352	„
58	„ 28	1,741	Rope parted. Grapnel lost. Bell rang.
59	„ 30	1,975	Bell rang. No strain above normal (*No. 4 Grapnel.*) On easing up on rope the pressure on the plungers ceased and the deflection fell to zero. Unsuccessful tow.
60	Aug. 1	2,215	Unsuccessful. Springs of grapnel too weak for the stiff ground in this locality.
61	„ 11	800	Unsuccessful.
62	„ 11	800	Bell rang. Cable not hooked.
63	„ 12	410	„ Cable hooked.
64	„ 12	370	„ „
65	„ 12	481	Unsuccessful.
66	„ 12	420	Bell rang. Hove up, and found that the coupling cover had slipped down shank and depressed pushes.
67	Sept. 5	420	Unsuccessful.
68	„ 5	420	„
69	„ 11	530	Bell rang. Cable hooked.
	1888.		No. 14.
70	May 25	1,480	Unsuccessful.
71	„ 25	1,548	„
72	„ 26	1,548	„
73	„ 26	1,548	„
74	„ 26	1,548	„
75	„ 30	1,420	Bell rang. Cable hooked.
76	June 1	1,619	„ „
77	„ 4	1,445	„ „
78	„ 4	1,445	Unsuccessful.
79	„ 5	1,445	„
80	„ 5	1,445	„
81	„ 8	1,586	„
82	„ 8	1,586	„
83	„ 9	1,395	„
84	„ 9	1,700	Bell rang. Indication due to stiff clay.
85	„ 14	1,908	„ Cable hooked. Fitted new contact plate.
86	„ 16	1,900	„ „
87	„ 17	1,900	„ „
88	„ 17	1,900	„ „
89	„ 20	1,575	Unsuccessful.
90	„ 23	1,575	„
91	„ 23	1,575	Bell rang. Cable hooked. Repair completed.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1888.		No. 15.
92	July 9	119	Unsuccessful.
93	„ 9	83	Cable hooked. Grapnel failed to indicate. Rucked up iron wires prevented depression of pushes.
94	„ 10	83	Unsuccessful. One plunger depressed and choked with stiff mud.
95	„ 10	583	Bell rang. Cable hooked.
96	„ 11	376	Cable hooked. Grapnel failed. Plunger heads too short.
97	„ 16	296	Unsuccessful.
98	„ 16	490	Bell rang. Cable hooked. Repair completed.
			No. 16.
99	July 18	562	Bell rang. Cable hooked.
100	„ 27	751	„ „ Repair completed.
			No. 17.
101	Aug. 9	103	Unsuccessful.
102	„ 9	103	Bell rang. Cable hooked. Strain rising at same time. Strain suddenly fell to zero, but indicator still deflected. Hove cable to bows.
103	„ 9	103	Bell rang. Cable hooked.
104	„ 9	103	„ „ Finished repair.
			No. 18.
105	Aug. 10	52	Unsuccessful.
106	„ 10	52	„
107	„ 11	52	„
108	„ 11	52	„
109	„ 11	52	Bell rang. Cable hooked.
110	„ 11	52	„ „
111	„ 11	52	Unsuccessful.
112	„ 12	50	Cable hooked. Grapnel failed. Repair completed.
			No. 19.
113	Sept. 29	85	Bell rang. Cable hooked.
114	„ 29	85	Unsuccessful.
115	„ 29	85	Cable hooked. Grapnel failed to indicate, owing to cable being twisted around shank. Cable broken on both sides.
116	„ 29	85	Cable hooked. Grapnel failed to indicate. The head of grapnel is too full. (See remark, page 248.*) Repair completed. * "This defect has," &c.
			No. 20.
117	Oct. 24	54	Unsuccessful.
118	„ 24	55	Bell rang. Hove up, and found crown plate and insulated disc torn away. Repaired damage.
119	„ 25	55	Bell rang. Cable hooked.
120	„ 25	55	„ „ Finished repair.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1888.		No. 21.
121	Dec. 17	20	Bell rang. Cable hooked.
122	„ 17	20	„ „
123	„ 17	20	„ „
124	„ 17	20	„ „
125	„ 17	20	„ „
126	„ 17	20	„ „ Finished repair.
	1889.		No. 22.
127	Mar. 28	—	Grapnel broken on rocky ground—shallow water off Brignogan. Repair completed with ordinary grapnel.
			No. 23.—*Grapnel No. 5.*
128	May 9	92	Bell rang. Hove up, and found a stone jammed between prong and shank, depressing one of the pushes.
129	„ 9	92	Cable hooked. Grapnel failed to indicate, owing to the covering of the coupling having slipped down shank and prevented depression of push.
130	„ 9	92	Bell rang. Cable hooked.
131	„ 12	92	„ „
132	„ 12	92	„ „ Finished repair.
			No. 24.
133	May 16	70	Bell rang. Hove up and found crown plate and insulated disc torn away. Ground exceedingly rough. Continued the use of grapnel, but, of course, not as an automatic one. Finished repair. May 17th: Refitted grapnel.
			No. 25.
134	May 17	11	Bell rang. Cable hooked.
135	„ 18	11	„ „
136	„ 18	11	„ „
137	„ 18	11	„ „ Finished repair.
			No. 26.
138	May 27	67	Bell rang. Cable hooked.
139	„ 27	67	„ „ Finished repair.
			No. 27.
140	June 8	33	Bell rang. Hove up, and found about 8 fathoms of the abandoned "'67" cable on grapnel.
141	„ 8	33	Bell rang. Cable hooked.
142	„ 8	33	„ „ (Piece of abandoned "'72.")
143	„ 8	33	„ „ Finished repair.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1889.		No. 28.—*Deviation.*
144	June 14	80	Bell rang. Cable hooked.
145	„ 24	80	„ „ } No indication whatever on dynamometer. When grapnel up, end of cable nearly slipping over prongs.
146	„ 26	54	„ „
147	„ 29	120	Unsuccessful.
148	„ 29	120	„
149	„ 29	120	„
150	„ 29	120	„
151	July 1	80	„
152	„ 1	80	„
153	„ 8	80	„
154	„ 8	80	„
155	„ 8	80	„
156	„ 8	80	„
157	„ 9	80	„
158	„ 9	80	„
159	„ 9	101	„
160	„ 9	101	„
161	„ 10	101	Bell rang. Hove up, and found push depressed.
162	„ 10	101	Unsuccessful.
163	„ 10	101	„
164	„ 10	101	„
165	„ 10	101	„
166	„ 11	101	„
167	„ 11	101	„
168	„ 11	101	Bell rang. Cable hooked.
169	„ 12	101	Unsuccessful.
170	„ 12	101	Bell rang. Cable hooked. Deviation finished.
			No. 29.
171	Aug. 12	1,800	Bell rang. Cable hooked.
172	„ 16	1,950	Unsuccessful.
173	„ 17	2,061	Bell rang. Cable hooked.
174	„ 24	1,976	„ „
175	„ 24	1,976	„ „
176	„ 25	1,869	„ „
177	„ 26	1,725	„ „
178	„ 27	1,725	„ „
179	„ 31	1,948	Unsuccessful.
180	„ 31	1,948	„
181	„ 31	1,900	Bell rang. Cable hooked. Finished repair.
			Note.—Tow No. 176.—10.10 a.m.: Lowered grapnel. 12.17 p.m.: Dynamometer showing rising strain. Began picking up, but, strain decreasing, and no indication being observed on instrument, paid out again. Tow 181.—Strain increased. Hove in. No indication on grapnel circuit. Paid out again.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1889.		No. 30.
	Dec. 9	—	Tested 670 fathoms length of Type A, old grapnel rope, shipped in London December, 1883. Insulation = 50 megohms. Joined up to grapnel.
182	„ 10	84	Cable hooked. Grapnel failed, owing to "rounding" having slipped down shank. Hove up a short piece of cable. (See note, p. 248.)
183	„ 10	84	Unsuccessful.
184	„ 11	84	Bell rang. Cable hooked.
185	„ 11	84	,, ,, Repair completed.
	1890.		No. 31.
186	Mar. 28	85	Unsuccessful.
187	„ 28	85	,,
188	„ 29	85	Bell rang. Cable hooked.
189	„ 30	85	,, ,, Repair completed.
			No. 32.
190	Apr. 8	57	Unsuccessful.
191	„ 8	57	Cable hooked. In this instance grapnel indicated after the dynamometer, the plunger springs being too strong. Fault; cable not severed. April 10th: Repair completed.
			No. 33.
192	May 5	102	Cable hooked. Bell rang after strain had risen. Heavy mass of sea-weed prevented the immediate depression of the plunger.
193	„ 5	96	Bell rang. Cable hooked.
194	„ 5	94	,, ,,
195	„ 5	94	Cable hooked. Grapnel on this occasion was unfortunately lowered with one of the pushes down, and was consequently used only as an ordinary one. Repair completed.
			No. 34.
196	May 27	66	Bell rang. Cable hooked.
197	„ 27	66	Unsuccessful.
198	„ 27	66	,,
199	„ 27	66	,,
200	„ 27	66	Bell rang. Cable hooked. Repair completed.
			No. 35.
201	May 28	45	Unsuccessful.
202	„ 28	45	Bell rang. Cable hooked.
203	„ 28	45	Unsuccessful.
204	„ 28	42	Cable hooked. Strain rising and indicator deflected almost simultaneously. Repair completed.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1890.		No. 36.
205	Sept. 6	58	Unsuccessful.
206	„ 7	72	Bell rang. Cable hooked.
207	„ 7	72	„ „
208	„ 7	83	„ „ Finished repair.
			No. 37.
209	Oct. 14	117	Unsuccessful.
210	„ 14	122	„
211	„ 14	122	„
212	„ 14	117	„
213	„ 14	117	Bell rang. Cable hooked.
214	„ 14	117	Unsuccessful.
215	„ 14	117	„
216	„ 14	117	Bell rang. Cable hooked.
217	„ 14	117	„ „ Repair completed.
	1890.		No. 38.
218	Oct. 16	58	Cable hooked. Strain increased and indicator deflected simultaneously.
219	„ 16	58	Bell rang. Cable hooked.
220	„ 16	58	Cable hooked. Grapnel failed to indicate.
221	„ 16	58	Unsuccessful.
222	„ 17	58	Bell rang. Cable hooked.
223	„ 17	58	Unsuccessful.
224	„ 17	58	Hove up, and found a short piece of cable—about 100 fathoms—on grapnel. Plunger springs rather too strong for this weak cable.
225	„ 17	58	Unsuccessful.
226	„ 20	90	Strain increased and indicator deflected simultaneously. Hove up a short piece of cable.
227	„ 20	90	Full deflection on indicator. Cable hooked.
—	„ 23	—	Finished repair.
228	„ 24	90	Bell rang. Cable hooked. Hove in some cable left during a former repair.
			In these shallow-water operations it occasionally happened that the grapnel did not indicate immediately when the cable hooked. This was owing to the strong springs having been used.
	1891.		No. 39.
229	Mar. 28	76	Unsuccessful.
230	„ 28	69	Indicator deflected. Cable hooked.
231	„ 28	67	„ „
232	„ 28	145	„ „ Finished repair.

No. of Tow.	Date.	Depth in Fathoms.	REMARKS.
			No. 40.
	1891.		*Repair in 10 fathoms. Rock. Ordinary grapnel used.*
233	May 7	200	Unsuccessful.
234	„ 9	210	„
235	„ 9	201	„
236	„ 9	201	„
237	„ 10	201	„
238	„ 10	219	„
239	„ 10	219	„
240	„ 10	219	„
241	„ 11	219	„
242	„ 11	141	Bell rang. Cable hooked and broken.
243	„ 12	141	Unsuccessful.
244	„ 12	141	„
245	„ 12	141	„
246	„ 12	202	„
247	„ 13	202	Deflection of 40° on indicator for half an hour. Probably towing the broken end.
248	„ 13	224	Unsuccessful.
249	„ 20	224	Bell rang. Cable probably parted.
250	„ 20	224	Bell rang. Cable hooked, but it slipped over grapnel at surface.
251	„ 20	224	Unsuccessful. Bell rang, but cable not hooked. Light springs, *imperfectly adjusted.*
252	„ 20	224	Unsuccessful.
253	„ 21	224	Deflection, 55°. Hard clay depressed plunger.
254	„ 21	224	Unsuccessful.
255	„ 21	224	Full deflection on indicator for an instant. Cable probably parted.
256	„ 21	184	Full deflection for an instant. Hove up short piece of cable.
257	„ 22	184	Deflection on indicator, 30°. Hove up short piece of cable.
258	„ 22	184	Unsuccessful.
259	„ 22	184	„
260	„ 22	184	„
261	„ 22	184	Full deflection on indicator. Short piece of cable hove in.
262	„ 22	184	Full deflection on indicator. Hove up. O.K. to eastward.
263	„ 23	184	Full deflection on indicator. Probably towed through the cable.
264	„ 25	184	Unsuccessful.
265	„ 25	184	Full deflection. Caused by stiff clay.
266	„ 25	184	Few fathoms of rotten cable hove up. Grapnel failed.
267	„ 25	184	Unsuccessful.
268	„ 25	184	„
269	„ 25	184	„
270	„ 26	184	„
271	„ 26	214	Full deflection. Short piece of cable hove up.
272	„ 26	214	„ „ „ „
			Cable being very rotten and reduced to bare core in places, fitted lighter springs to the grapnel. Springs respond to a pressure of 25 lbs.
273	„ 26	214	Unsuccessful.
274	„ 26	214	Few fathoms of cable hove in.

No. of Tow.	Date.	Depth in Fathoms.	Remarks.
	1891.		
275	May 26	214	Full deflection. Cable parted or slipped over grapnel.
276	„ 27	214	„ Short piece hove in.
277	„ 27	214	„ Cable parted near the bottom.
278	„ 27	214	„ „ „ „
279	„ 27	214	Unsuccessful.
280	„ 27	214	Full deflection. A few fathoms of cable hove in.
281	„ 27	214	„ „ „ „ „
282	„ 29	239	Full deflection. Cable parted 50 fathoms from surface.
283	„ 30	240	„ „ 190 „ „
284	„ 30	240	Unsuccessful.
285	„ 30	240	Full deflection for about 30 seconds. Cable probably parted.
286	„ 30	240	Full deflection. Cable probably parted.
287	„ 30	240	„ Cable parted.
288	„ 30	240	„ Short piece hove in.
289	June 1	240	Unsuccessful.
290	„ 1	240	Full deflection. Cable parted 50 fathoms from surface. Short piece hove in.
291	„ 1	240	Full deflection. Cable parted. Short piece hove in.
292	„ 1	240	„ „ „ „
293	„ 2	240	Full deflection. Cable parted 90 fathoms from surface. A few fathoms hove in.
294	„ 2	240	Full deflection. Cable parted at surface. A few fathoms hove in.
295	„ 9	368	Unsuccessful.
296	„ 9	368	Full deflection Cable parted 130 fathoms from surface. A few fathoms hove in.
297	„ 10	368	Full deflection. Cable parted.
298	„ 10	368	„ „ 250 fathoms of rope out.
299	„ 10	368	„ for two minutes. Cable probably parted.
300	„ 11	382	„ Cable parted. 240 fathoms of rope out.
301	„ 11	382	Unsuccessful.
302	„ 11	382	Full deflection. Cable parted. 270 fathoms of rope out.
303	„ 11	382	„ „ 130* „ „
304	„ 11	382	„ „ 150* „ „
305	„ 16	382	Deflection, 60°. „ 50* „ „
306	„ 16	390	„ „ „ 240 „ „
307	„ 16	415	Full deflection on indicator. Buoyed grapnel 70 fathoms from bottom.
308	„ 16	415	Full deflection on indicator. Buoyed cable on the bottom.
309	„ 23	418	Full deflection on indicator. Cable parted. 310 fathoms of rope out.
310	„ 23	418	Full deflection, but dynamometer had begun to show a rising strain. (See note respecting shape of grapnel, p. 248.) Cable parted. 230 fathoms of rope out. Short piece of cable hove in.
311	„ 23	418	Full deflection. Cable parted. 290 fathoms of rope out.
312	„ 24	—	Unsuccessful.
313	„ 24	—	60 fathoms of rotten cable hove in. Proceeded to eastern cable buoy. Spoke shore. Then steamed to the westward.
314	„ 25	680	Full deflection. Cable hooked. Buoyed bight.
—	„ 27	—	Finished repair.

* Short pieces hove in.

SUMMARY.

Grapnel indicated "Cable hooked"—		
Before dynamometer	137	
With	4	143
After	2	
Grapnel indicated accidents to apparatus	10	
False indications	5	
Grapnel failed to indicate	15	
Doubtful indications	7	171
Accidents not indicated	3	
*Unsuccessful tows	131	
Total number of tows		314
" " when cable hooked ...		158
" " " *not hooked* ...		156

A careful record has been kept of every tow, and none of the failures have, as far as I am aware, been hidden. A few accidents and repairs to the grapnel rope are not recorded in the paper, but will be noted in a future communication.

I have not thought it necessary to enter the time between the indication given by the grapnel and that shown on the dynamometer; in fact, it would convey nothing if I did, for now, as a rule, picking up is begun, or should be begun, as soon as the electrician is satisfied with the indication given by the grapnel.

Intermittent depressions of the pushes are simply noted as being probably due to stony ground, but when cable is hooked the pushes are usually driven in gradually and steadily. This, of course, depends in a measure on the strength of the spiral springs against which the pushes work.

For ordinary work these springs respond to about 90 lbs., but when the cable is much deteriorated weaker springs are used, about 20 lbs. only being sometimes necessary.

If there be any doubt as to whether the indication is due to cable or not, it is only necessary to slack up on the rope so that the grapnel may drop *shank down*, when the indication will cease, and then heave away again, when, as soon as the grapnel is tilted, the push will be once more driven in, if the cable is there.

* Tows either in the gap, or not across the line of cable.

It occurs to me as being worth mentioning, that in paying out the grapnel rope for the first time, on no account should a "jockey" be used. The jockey has the effect of bulging the outer layer. The rope will adjust itself after clearing the drum and sheaves, and the oftener it is used the "kinder" it becomes.

When cables are in close proximity, and it is desired that their respective positions be ascertained, this grappling apparatus is especially valuable. On many occasions the wrong cable has been hooked and slipped—by paying out the rope "with the run"—without being dragged out of position. As a "searcher" for torpedo lines this grapnel and rope would prove of especial value.

With careful and intelligent nursing this valuable addition to our ordinary system of grappling must prove of no inconsiderable value, more especially in deep water.

The grapnel requires overhauling occasionally. The ship's jointer usually takes off the crown plate and examines the insulated disc and the pushes whenever the grapnel is taken inboard, and careful tests of the rope and grapnel are taken from time to time during the tows. I have been in the habit of using a switch so as to connect instantly to bell, indicator, Wheatstone's bridge, or telephone. The use of the latter is both interesting and useful, as it sometimes enables the listener to form an idea as to the nature of the bottom.

The insulation of the circuit will sometimes fall in consequence of the india-rubber disc being punctured again and again in the same place; but there is no difficulty in working the grapnel even when the resistance falls below 1,000 ohms, but by simply turning the disc a few degrees a fresh surface is presented and the old punctures close up.

The following notes are extremely interesting, showing as they do the satisfactory work performed with this grappling apparatus several years ago:—

July 27th, 1885.—1st Tow: Bell indicated "Cable hooked" at 9.3 a.m. Strain shown on dynamometer, 4 tons, at 9.45, when 475 fathoms of rope in. Depth, 710 fathoms. Immediately on cable being cut at bows bell ceased ringing.

July 29th.—2nd Tow: Bell indicated "Cable hooked" at

12.25 p.m. Dynamometer showed extra strain at 12.48 p.m. Depth = 1,375 fathoms.

July 29*th.*—3rd Tow: On this occasion the strain rose, but bell in circuit remained silent. Strain due to stiff ground.

July 30*th.*—4th Tow: Bell indicated "Cable hooked" at 7.18 p.m. No indication on dynamometer. Cable hooked too near the end.

July 31*st.*—5th Tow: Bell indicated "Cable hooked" at 5.30 p.m. Dynamometer showed extra strain at 7.50 p.m., or 2 hours and 20 minutes later, when ship was, of course, considerably past the line of cable. The indication was not acted on promptly, and the cable consequently slipped over the grapnel. On heaving grapnel to the surface, compound was found on the plungers. Depth = 1,666 fathoms.

August 3*rd.*—6th Tow: Bell gave warning at 5.32 p.m, when heaving in was immediately begun. At 6 p.m. dynamometer showed increasing strain. Depth = 1,146 fathoms. On this occasion cable slipped over grapnel.

August 4*th.*—7th Tow: This time the indications showed a fault in rope or an injury to grapnel. Unfortunately, the latter proved to be broken, the section at the fracture showing an imperfect casting. The grapnel had, however, done good service.

ABSTRACTS.

ANON.—THE LAUFFEN-HEILBRONN TRANSMISSION.

(*Elektrotechnische Zeitschrift, No.* 2, 1893, *p.* 18.—*I.*)

This interesting example of power transmission, designed and carried out by Oscar v. Miller, was begun on Sept. 15th, 1891, and the first current was switched on on Jan. 10th, 1892; it is calculated to supply, when working at full output, 19,200 8 C.P. lamps, or their equivalent in motors or arc lamps. Its chief interest lies in the use of three-phase motors and dynamos, one of the latter having been utilised for the well-known Lauffen-Frankfort transmission. The dynamos give 4,000 amperes at 50 volts, and are driven by water power, the turbines being coupled through level gearing; and the current is passed into a transformer which raises its pressure to 5,000 volts, at which pressure it enters the three bare copper wires which, insulated by means of oil insulators on high poles, form the circuit by which the energy is transferred to Heilbronn, where the pressure is reduced to 1,500 volts and fed into a primary network on which the secondary transformers are placed, by means of which the pressure is reduced to 100 volts in the secondary network. The objects of this arrangement are said to be—(1) To use the very safe pressure of 50 volts in the dynamos; (2) to use very cheap and light wires for the transmission in spite of the great distance; (3) to use a cable network of moderate insulation and yet inconsiderable section; (4) to use large, and therefore cheap and efficient, transformers.

The turbines give 330 H.P. with 3·85 m. fall, and their speed, regulated by hand, is 35 revolutions per minute. The dynamos have stationary armature and massive rotating field magnet, and the winding of the armatures is formed of massive copper bars; the speed is 150 revolutions per minute, the number of poles 32, the output 4,000 amperes at 50 volts. The excitement calls for no remark; each dynamo is capable of exciting three alternators. To prevent the alteration of speed at light load caused by switching in or out large motors, the superfluous electrical energy is used in the Portland Cement Works during the day for the purpose of drying clay, and a great economy has been thereby effected.

A considerable amount of attention has been paid to the question of motors which are in extensive use. These are, as is well known, self-starting, and are of simple construction. The larger ones are switched on and off through liquid resistances, which prevent a complete closing or breaking of the circuit during some 15 to 20 seconds, in order to prevent variations of speed, and hence of the volts. As regards price of power supplied, the price for light is about equivalent to 9d. per Board of Trade unit, while for motors and heating purposes the price is fixed at 4d., and there is also a sliding scale which gives reductions to large consumers. The total cost of the works was about £13,000, and in November last

there were coupled about 3,400 glow lamps, 8 C.P., 20 arcs, and 11 motors averaging about 3 H.P. each, and the number was increasing rapidly.

F. UPPENBORN—THE BARMEN CENTRAL STATION.

(*Elektrotechnische Zeitschrift, No.* 1, 1893, *p.* 1.)

This station is one of those carried out by Messrs. Schuckert & Co., and is one of those in which extensive use is made of storage cells. It was started on December 6th, 1888; the engines and the buildings being supplied by the municipality, while Messers. Schuckert carried out the electrical work to the designs of the chief engineer, Mr. H. Müller. This was the first station in which the system was adopted of having the cells constantly in parallel with the dynamos and the lamps supplied, using charging and discharging switches of the usual type. The capacity of the station was originally intended to be 10,000 30-watt lamps; but this has only been found to suffice for part of the town, and five new accumulator stations are in contemplation. The number of lamps coupled is 216 arcs, and 5,333 glow lamps of 10 to 25 candle-power in 141 installations. Lancashire boilers are used, supplying steam to horizontal compound non-condensing engines of 100 H.P. each, running 110 revolutions a minute, and driving two dynamos each, by means of belts. These dynamos are of the well-known Schuckert type, with four poles, and have disc armatures, the speed being 500 revolutions per minute. The accumulators are coupled to the dynamos on a three-wire system, each pair of dynamos being coupled in series, the central point connected to the centre of the 240-volt battery, and the two outer terminals through the charging switches to the two outer terminals of the cells, which discharge through similar switches into the outer wires of the three-wire system, to the middle wire of which the central point is connected. Provision is made for running the dynamos direct on the mains. The accumulators can discharge at 220 amperes, and the switches are arranged with resistances between the contacts, so as not to short-circuit when passing over. The omnibus bars are connected to the feeders, the middle wires direct, the outer through regulating resistances, which are automatically adjusted by means of apparatus worked by pilot wires from the feeding points; but the regulation is said to be so good that even the cell regulators are only in use at the time of maximum load.

The cables form a system extending over an area about one mile long and one-third of a mile broad, and are calculated on a basis of 10,000 30-watt lamps for an efficiency of 90 per cent.; they are lead-covered and armoured cables, and are laid on an average 1 metre deep, in sand; they are protected where necessary by drawing through iron piping. There has been no interruption of the supply since the start in December, 1888, but the machinery runs on an average about 10 hours a day only, the cells supplying the rest of the energy required. The double cells, however, do not appear to have been entirely satisfactory, and were replaced by single cells of improved type later on; but the general satisfaction is shown by the fact that the new work is to double the output of the station. As regards com-

mercial points, the capital expenditure up to December 31st, 1891, was about £37,500, and the price charged per unit is 10d. (nearly); the capital expenditure per lamp supplied was about £8, and would be about £5 10s. when the station is in full work.

T. FISCHER-HINNEN—THE REACTION OF THE ARMATURE CURRENT ON THE MAGNETIC FIELD.

(*Elektrotechnische Zeitschrift*, *No.* 5, 1893, *p.* 53.)

This paper is an attempt to deal, by means of approximate formulæ, with the question of armature reactions, both in order to allow for them and to avoid them.

The author gives as a formula for the angle of the displacement of the brushes,

$$\mathrm{Tan}\, \alpha = \frac{Z_a}{Z},$$

where Z_a is the number of lines of force of the armature obtained by dividing the magnetising force of the armature winding by the sum of the magnetic resistances of the parts of the path, and Z is the total number of lines per pole producod by the magnets; and he defends this formula by instances of practical experience. He gives directions for calculation of the compensating winding for increasing the magnet strength to make up for direct armature action; he holds that the position of this winding makes no difference, and hence objects to the Thomson-Houston arrangement of placing it over the armature.

As regards diminution of armature reactions, the author points out that to avoid heavy machines the opportunities for these effects have been increased; and he alludes to the method of putting saw-cuts in the pole-pieces to prevent transverse magnetisation, quoting an experiment with a Manchester machine, which ran with violent sparking at 10 volts and 800 amperes, and required 60° brush displacement. The angle was reduced to 30° and the sparking was much diminished. A second method is alluded to—that of placing a coil over the armature so as to neutralise its effect upon the poles. If this coil has the same ampere-turns as the armature, distributed evenly over the pole-face, the armature reaction disappears. An experiment with the above-mentioned Manchester machine showed that the machine was absolutely sparkless at 10 volts and 1,000 amperes, and had no displacement of brushes. He concludes, therefore, that in this manner the armature reaction cannot only be entirely eliminated, but the output of a machine can be sensibly increased.

A. A. CAMPBELL SWINTON—EXPERIMENTS WITH HIGH-FREQUENCY ELECTRIC DISCHARGES.

(*Phil. Mag.*, *Vol.* 35, *No.* 213, *p.* 142.)

The author of this paper has succeeded in passing through his body, from hand to hand, sufficient electricity to bring the filament of an ordinary 5-candle-power 100-volt incandescent lamp very nearly to full incandescence, and found that practically no sensation resulted from the transmission. The apparatus used consisted

of a large induction coil coupled to three Leyden jars whose disruptive discharge excited the primary of a high-frequency induction coil. The secondary of this induction coil had its terminals in two brass balls, to which the wires were led through glass tubes filled with oil. One of the terminals may be grasped with impunity, and hardly any sensation is felt; and while thus in contact sparks will pass to any conductor placed near enough. If the free hand, holding a metal rod, be approached to the terminal of an incandescent lamp, as described above, whose other terminal is earthed, the lamp is first filled with phosphorescent light, then sparks pass and the lamp glows, and on making contact the lamp reaches very nearly its full incandescence. The author believes that this result is due, not to the passage of the full normal current through the body (its effect being not felt owing to the exceedingly high frequency), but to the fact that the amperes taken are actually much less, owing to the crowding of the current to the outer surface. The virtual resistance is thus very high, and increases the voltage of the lamp; and thus a much smaller current enables the lamp to receive enough energy to produce incandescence. This explanation is confirmed by the fact that while the lamp was glowing sparks passed between its terminals, thus proving that a very high P.D. existed, of the order of thousands of volts.

Other curious results were obtained. If the connection from the secondary coil to the lamp were made by a wire, instead of utilising the body, the lamp reached more than its normal candle-power; thus showing that the body offers resistance to the passage of the current. To estimate this resistance the thumbs of the connecting man were approached, and sparks about ¼ inch long passed, showing a P.D. of some thousand volts. If the hands were brought into contact, so as to short-circuit the man, the lamp became appreciably brighter. Placing the body in parallel with the lamp between one terminal of the high-frequency coil and earth, the lamp was reduced to less than half its brilliancy, the two impedances being apparently about the same. If one terminal of the lamp were connected to the coil, and the other were touched by a piece of metal held in the hand of the insulated or uninsulated operator, the lamp glowed. The effect in this case is due to the capacity of the gentleman in question. These experiments were made with the other terminal of the high-frequency coil free: if it were earthed or touched by another operator, the brilliancy of the lamp was diminished. Again, the incandescence was diminished if the lamp was connected directly across the terminals of the lamp; and if the operator occupied a similar position the sensation was quite unbearable. The author concludes, therefore, that capacity has much to do with the results obtained, and that the physiological effects of electric currents are not necessarily proportional to their heating power.

F. HEERWAGEN—A NEW METHOD OF MEASURING THE SPECIFIC INDUCTIVE CAPACITY OF LIQUIDS.

(*Wiedemann's Annalen, Vol.* 48, *No.* 1.)

The author uses for this purpose a differential electrometer, in which a common

axle carries two needles moving in two separate sets of quadrants vertically over one another. The needles, the case, and one pair of quadrants in each instrument are connected together and to a certain point in the circuit, and if the remaining pairs of quadrants be attached to other points in the circuit, such that there is no deflection of the instrument, the ratio of the squares of the differences of potential between these points and that which is in contact with the case is the same as the ratio of the sensibility of the electrometers.

If W_l / W_f be this ratio for air as dielectric, and W_L / W_F be the ratio when the lower half of the instrument is filled with the liquid under examination, the specific inductive capacity of the liquid is given by the formula,

$$K = \left(\frac{W_L}{W_F}\frac{W_f}{W_l}\right)^2.$$

Alternating currents must be used to prevent electrolysis taking place; and the upper electrometer should, the author finds, be more sensitive than the lower one. He also calls attention to the fact that the resistance of the suspension prevents needles and case from being accurately at the same potential, and he recommends the insertion of an artificial resistance equal to that of the suspension between the case and quadrants and the point of contact with the circuit.

Lengthy and elaborate experiments are described fully in the paper for determining the specific inductive capacity of pure water, which is given by the author, after allowing for all corrections, as,

$$K = 79{\cdot}56 \text{ at } 20{\cdot}75^\circ \text{ C.}$$

H. O. G. ELLINGER—THE REFRACTIVE INDEX OF ELECTRIC WAVES IN ALCOHOL.

(*Wiedemann's Annalen, Vol.* 48, *No.* 1, *p.* 108.)

The experiments were performed with alcohol of 96 per cent. strength, which was placed in a prism-shaped wooden container, 3·25 ft. high, 3·8 ft. long, with an angle of 8° 16′ at the apex, and holding 90 litres of alcohol. The Hertz spark was found to take place in the secondary circuit when the primary reflectors made an angle of 33° with one another. The value of the refractive index works out at 4·9; and the author remarks that the value accords fairly well with theory, as the specific inductive capacity is known to be not far off $(4{\cdot}9)^2$.

A. HEYDWEILLER—THE DETERMINATION OF HIGH POTENTIALS.

(*Elektrotechnische Zeitschrift, No.* 3, 1893, *p.* 29.)

The author developes equations given by Kirchhoff for the rate of fall of potential at the place of discharge for different values of radius of balls, sparking distance, and potential, and evolves a means of calculating the P.D. at which sparking will take place in any particular case. He tested the accuracy of his

predictions by a kind of torsion-balance mirror electrometer, and the following is an abridgement of his table:—

P.D. of Discharge in Kilovolts.
Similar spherical electrodes: Radius = r, sparking distance = d.
Bar. = 745 mm.; therm. = 18° C.

r = 2·5 cm.			r = 1 cm.			r = 0·5 cm.			r = 0·25 cm.		
d.	V, obs.	V, calc.	d.	V, obs.	V, calc.	d.	V, obs.	V, calc.	d.	V, obs.	V, calc.
0·5	18·4	17·8	0·1	4·7	4·1	0·1	4·8	4·5	0·1	4·8	4·8
0·75	26·1	25·9	0·2	8·1	7·9	0·2	8·4	8·4	0·2	8·4	8·6
1·0	32·8	33·4	0·3	11·4	11·4	0·3	11·4	11·9	0·3	11·3	11·4
1·25	39·7	40·5	0·5	17·5	17·9	0·5	17·3	17·7	0·5	15·7	15·5
1·5	46·2	47·2	0·7	23·2	23·6	0·7	22·0	22·1	0·7	18·3	18·1
...	...	...	1·0	31·3	30·9	0·9	25·6	25·6	1·0	20·2	20·5
...	...	...	1·4	38·6	38·7	1·5	31·8	32·5	2·0	23·2	23·8
...	...	...	2·0	47·5	47·5	...	...	...	6·0	25·7	26·0

Note that the values vary with air temperature and pressure. The values may be used for calibration of instruments, being right within 1 per cent. For small electrodes no distances over 1 cm. should be used, larger spheres being used for greater pressures. The experiments only apply to slowly raised pressure.

FERNANDO SANFORD—A NECESSARY MODIFICATION OF OHM'S LAW.

(*Phil. Mag., Vol.* 35, *No.* 212, *p.* 65.)

The author has determined the resistance of a copper circuit containing various dielectrics, and has come to the conclusion that the resistance depends to an appreciable extent on the dielectric. The copper circuit in his experiments took the form of a double-ended cylinder, having a wire attached to the centre of the lower end and passing through an insulating bush in the upper one. This was filled with various dielectrics, and curves were taken of its resistance at various temperatures over a range of 10° C., by a bridge capable of measuring resistances within 0·00001 ohm with a fair degree of accuracy. Air and petroleum were compared by means of a series of observations extending over two months, the dielectrics being changed five times, and the results were perfectly consistent. The author found that if the conductivity of the copper in air be taken as unity, its value varies from 1·0018 in petroleum to 0·9973 in a mixture of wood alcohol and petroleum—a range of nearly half per cent.

He finds that it is probable that only the part of the dielectric in direct contact was concerned in the effect, as until it was thoroughly cleaned and dried the cylinder did not return to the value unity. A silver wire shows the same effect to a less marked degree.

W. JAEGER—NOTE ON THE PURIFICATION OF MERCURY.

(*Wiedemann's Annalen*, *Vol.* 48, *No.* 1, *p.* 208.)

The author describes the method of purification employed at the Reichsanstalt for the preparation of mercury for standards of resistance, &c. Quicksilver from the Idria mines is used—a metal which is of good quality, and may be used for all ordinary purposes after simple filtration. In order to get rid of the heavy metals, the mercury is twice distilled in a vacuum, during which operation great care must be exercised to avoid all traces of grease or dirt on the rubber tubing or glass used. After distillation, since metals such as zinc might still remain, the mercury undergoes a further purification by electrolysing a solution of mercurous nitrate. The mercury so obtained is found to be of remarkable purity.

W. E. SUMPNER—THE DIFFUSION OF LIGHT.

(*Phil. Mag.*, *Vol.* 35, *No*, 213, *p.* 81.)

The author has experimented on the reflecting, absorbing, and transmitting powers of various substances, and his tables are contained in a paper which deals generally with the subject of illumination. They are as follows:—

REFLECTING POWERS.

	Per Cent.		Per Cent.
White blotting paper	82	Deep chocolate paper	4
White cartridge paper	80	Plane deal (clean)	40–50
Tracing cloth	35	„ (dirty)	20
Tracing paper	32	Yellow cardboard	30
Ordinary foolscap	70	Parchment—	
Newspapers	50–70	One thickness	22
Tissue paper—		Two thicknesses	35
One thickness	40	Yellow painted wall (dirty)	20
Two thicknesses	55	,, ,, ,, (clean)	40
Yellow wall paper	40	Black cloth	1·2
Blue paper	25	Black velvet	0·4
Dark brown paper	13		

The author points out that if the reflecting, absorbing, and transmitting coefficients of a material be called η, a, and τ, then the relation $\eta + a + \tau = 1$ must hold; and evolves for the absorption coefficient a the formula $a = (1 - \eta)\dfrac{K_0 - K_1}{K_0}$, where K_0 is the candle-power of the source of illumination used, and K_1 its candle-power after surrounding it with an envelope of the substance under examination; and he gives the following absorptions:—

ABSORBING POWERS.

	Per Cent.		Per Cent.
White blotting paper	13·8	Tracing cloth	15·0
White cartridge paper	12·2	Tracing paper	7·0

The transmitting qualities were also determined:—

TRANSMITTING POWERS.

	Per Cent.		Per Cent
Blotting paper	9·2	Tracing cloth	54·4
Cartridge paper	11·2	Tracing paper	76·0

The author points out that the sum of these coefficients is 104–105 per cent., and he thinks that the discrepancy may be explained if the law of the cosine does not exactly hold—an explanation which he thinks not improbable.

R. ARNO—A ROTATING ELECTRIC FIELD, AND ROTATIONS PRODUCED BY ELECTROSTATIC HYSTERESIS.

(*Elektrotechnische Zeitschrift, No.* 2, 1893, *p.* 17.)

In Professor Ferraris's classic researches on rotation produced electro-dynamically by alternate currents, he showed that an iron cylinder will rotate in a magnetic field, even if so slit that eddy-currents cannot take place in the body of the material; and he explained the phenomenon as a result of magnetic hysteresis. The author has extended the experiment to the field of electrostatics. He observes that since condensers heat there must be electrostatic hysteresis, and if a body were placed in a rotating electric field it should rotate in the same direction as the field. He therefore suspended a cylindrical piece of mica between pairs of quadrants formed of quarter cylinders of brass, connected each to the opposite one, and charged these by means of E.M.F.'s in quadrature, thus producing a rotating electric field. He obtained these E.M.F.'s by sending a very high pressure alternating current through a circuit containing an inductionless resistance of very high value (formed of a tube of distilled water 3·5 mm. in diameter and of variable length) and a condenser of small capacity; the E.M.F.'s taken from the terminals of the resistance and those of the condenser are in quadrature. A commutator between these and the quadrants enables the field to be made to rotate in either direction. The mica cylinder was then found to rotate when the current was started, and to reverse on switching over the commutator. The same phenomenon was observed with paraffined paper, glass, ebonite, wax, and other insulators.

The author then constructed a little motor on this principle: it was found to work best with equal potential differences on the two sets of quadrants; and when this voltage, measured by a Thomson electrostatic voltmeter, was found to be 3,800 volts, the water resistance in this experiment having a resistance of 13·5 megohms, the speed of the ebonite "armature" was 250 revolutions a minute.

F. LUCAS—TRANSFORMER (DIRECT TO ALTERNATING).

(*La Lumière Electrique, Vol.* 46, *No.* 45, *p.* 274.)

Since the introduction of triphase currents, the transformation of continuous currents into triphase, two-phase, or ordinary alternating currents has been effected with facility.

Mr. F. Lucas has recently devised a method of transforming a continuous current from any source whatsoever, into either simple alternating currents, or multiphase currents with any desired frequency.

The apparatus consists of two copper or silicon bronze rings, each having an odd number of equally spaced radial arms. These two rings are mounted on the same spindle, and separated from one another by a disc of insulating material, and so fixed that the arms on one side coincide with the spaces on the other. Connections are made in the following manner :—The poles of the continuous-current source are connected to brushes making contact with the two rings. The ends of the circuit destined to carry the alternating current are connected to two brushes, each wide enough to make contact on either set of arms alternately, and placed diametrically opposite to one another. It will be seen that neither brush can make contact on both sides simultaneously, since the arms on one side coincide with gaps on the other. It will also be evident that when the top brush is bearing on one side the bottom brush will be bearing on the other side, as each set is made up of an odd number of arms. If the spindle be rotated, an alternating current will be produced, since the brushes alternately make contact on arms of different sign.

If n = the number of each set of radial arms, then the number of reversals per revolution = $2n$. If ω be the angular velocity of the arms, then the duration of each period $= \frac{\omega}{n}$, and is therefore proportional to angular velocity, and inversely proportional to the number of arms. There is, however, a period of short-circuit at every reversal, and in order to make this period as short as possible Mr. Lucas found it necessary to give the brushes a gentle rocking motion.

By suitably arranging the brushes it is possible to obtain diphase and triphase currents.

C. DECHARME—MOVEMENTS OF A MAGNET FLOATING ON MERCURY CARRYING A CURRENT.

(*Comptes Rendus, Vol.* 115, *No.* 18, *p.* 651.)

In carrying out these experiments the author employed a light magnetic needle 3 or 4 cm. long and pointed at both ends.

This needle was placed in a bath of absolutely pure mercury, through which an electric current was sent by dipping two platinum wires into it.

The magnet was found to rapidly move in different directions, according to the relative positions of the points dipping into the mercury, with the poles of the magnet. These positions offered a considerable number of combinations, some of which are here given.

1. The simplest case is that in which the points dip into the mercury, on each side and at equal distances from the needle, in a line at right angles to its axis, the positive pole being on the left and near the south pole of the magnet (at rest in the magnetic meridian); the needle was found to quickly move in a direction at right angles to the path of the current, and, if it had not passed outside the field of the current, to return in the same direction, with a slow motion at first, then to move quicker, then to settle after one or two oscillations in its position of equilibrium.

2. When the points dip in the mercury, in a line at right angles to the axis of the needle, but on the same side, the negative pole being near the south pole; the needle in this case, after having quickly moved in a direction at right angles to that of the current, returned by a path almost parallel to itself, to finally settle between the platinum points in the manner above described.

3. When the points dip into the mercury in a line parallel to the axis of the needle, it will move away, at first in a direction parallel to that of the current, and then return by a circular motion, to finally place itself after a quarter of a revolution in a position of equilibrium.

4. A more complex movement is produced when the points dip into the mercury in a line at right angles to the axis of the needle, but on the same side, the south pole being on the right side of the current; for example, when the points are both to the right of the needle and the negative pole of the current is near the south pole: in this case the needle, after having moved out far, returned after half a revolution to set itself in the normal position.

5. Finally, the most complex case is that in which the points dip in the mercury on each side of the needle, in a line at right angles to its axis, the negative pole being to the left of the south pole. To get into its normal position the needle at first moved out far, and then returned after half a revolution to settle into a position of equilibrium.

It will be evident that by reversing the poles of the current inverse actions to the preceding will be obtained.

C. E. GUILLEAUME—THE TEMPERATURE COEFFICIENT OF MERCURY.

(*Comptes Rendus*, *Vol.* 116, *No.* 2, *p.* 51.)

The author gives the following formula, for the true resistance of mercury, as a function of the hydrogen thermometer:—

$$(1.)\ R_T = R_0\,(1 + 0{\cdot}0008881\ T + 0{\cdot}00000101\ T^2);$$

this having been established from 64 series of observations, taken at equidistant points on the scale between 0° and 62°.

The author points out how very closely his formula agrees with that lately published by Messrs. Kreichgauer and Jäger—

$$(2.)\ R_T = R_0\,(1 + 0{\cdot}0008827\ T + 0{\cdot}00000126\ T^2);$$

this being a mean result of tests made on five mercury standards between 14·7° and 28·2°. The values calculated by formulæ (1) and (2) cross at a temperature of about 22°. This very close concordance between the two formulæ is all the more remarkable, that the observations were made on mercury purified by different processes, contained in the author's case, in hard French glass, with Benoit contacts; Messrs. Kreichgauer and Jäger having used Jena glass for containing the mercury, the contacts being fused into the glass.

The author considers that the close agreement between these formulæ is in no way due to mere coincidence, owing to the great number of observations made, and should tend to inspire confidence in carefully manipulated mercury standards.

J. P. ANNEY—THE GENOA POWER STATION.

(*La Lumière Electrique*, *Vol.* 47, *No.* 8, *p.* 351.)

La Société de l'Acquedotto de Ferrari-Galleria has been formed with the object of supplying water, as well as hydraulic power, to the inhabitants of Genoa. A lake has been made in the mountains, at a distance of 30 kilometres from Genoa, at a height of 550 metres above the sea level, and capable of supplying 500 litres of water per second. In order to minimise this great fall, three intermediate reservoirs have been constructed, thus reducing the final fall to 180 metres. The first reservoir is 112 metres below the opening of the main tunnel, and it is here that the electrical generating station was built, the gross power available being

$$112 \times 500 = 560 \text{ kilogrammetres,}$$

or 746 horse-power. The second reservoir is 108 metres below the first, the gross power in this case being therefore about 720 horse-power. The third reservoir is 144 metres below the second, 960 horse-power being here available. The distance of the generating station to the most distant motor is about 30 kilometres. Preliminary tests were made from the first station, called the "Galvani Station," on a series circuit of motors with great success, which eventually led to the erection of two other stations. The highest one is called the "Pacinotti Station," and the second one the "Volta Station." The motors used are all connected in series, and arrangements made for maintaining a very constant current with a varying potential at the station, depending on the number of motors in circuit, sometimes the voltage varying from 450 volts to 6,000 volts in a very short interval of time.

"Galvani Station."

At the Galvani Station two Thury dynamos are used, coupled direct to one Reiter turbine, working at a normal speed of 475 revolutions. These dynamos have six poles, the magnets and yokes being wholly of wrought iron. The armatures are drum-wound, and designed for a maximum output of 1,100 volts, 47 amperes, at a speed of 475 revolutions.

The limits of speed are from 20 revolutions to 475 revolutions, depending on the load.

The guaranteed efficiency was 90 per cent. for the dynamos. Actual tests at the station gave a little above 91 per cent. These tests were, however, made with instruments which were not very reliable for great accuracy.

"Volta Station."

This station has been in operation since December, 1891. The generating plant consists of four groups of machines. Each group consists of one 140-horse-power turbine, to which are coupled two Thury dynamos, designed for an output of 1,000 volts, 47 amperes. The eight dynamos are connected in series, and are, under these conditions, therefore capable of supplying current at a maximum potential of 8,000 volts. In this station the speed of the turbines is maintained constant, the regulation being effected by varying the excitation of the dynamos, which are separately excited from a machine driven by a special turbine, the speed of which is varied by an electric regulator. These turbines were supplied by MM. Faesch and Piccard, of Geneva.

Special precautions were taken for obtaining a sufficiently high insulation resistance on the dynamos to withstand a voltage which at times reaches 8,000 volts. The cores of the armatures were covered with two very thick layers of mica. Care was also taken to prevent sparking from the armature winding to the pole-pieces, the latter being covered by a screen of insulating material.

The bed-plates of the machines were also insulated from earth. Carbon brushes are used to minimise sparking at commutators, and have worked with great satisfaction.

The exciting dynamo is common to all generators, and is coupled direct to a 12- to 15-horse-power turbine, which works without a fly-wheel, and its moment of inertia is made as small as possible. The armature is only 180 mm. in diameter; moving parts being made as light as possible, in order that they may at once respond to the governing of the turbine.

The exciter has a very powerful field, and in order to maintain this as strong as possible it is separately excited from a small dynamo, also used for local lighting.

"Pacinotti Station."

This last station was opened November 23rd, 1892. The machine room is large enough to contain six groups of machines, each capable of developing 140 horse-power. Four of these groups are now in operation.

Piccard turbines are used, each working two Thury dynamos, designed for 1,000 volts, 45 amperes, 475 revolutions, and are capable of working up to 50 amperes.

All the dynamos are connected in series, the regulation having here been greatly simplified.

Separate excitation has been discarded, in this case a system of auto-excitation of the dynamos in series being used. The turbines have no fly-wheels, and the dynamos are so designed that the inertia of the armatures is small as compared to their power. For this reason automatic regulation is obtained, for as the resistance of the circuit increases, so the current diminishes, the motive effort is diminished, and the speed of the turbine consequently increased.

On the other hand, if the current increases, the motive effort is increased, and the turbine speed falls. Mr. Thury has taken care not to saturate the fields of the dynamos, which greatly facilitates this regulation, the motive effort actually increasing with the square of the current.

An auxiliary regulator is used to correct small variations in the current.

With regard to efficiency, that of the generators and six-pole motors was guaranteed at 90 per cent. This was exceeded by 1 to 2½ per cent., according to output. For the other motors the efficiency varied from 85 to 89 per cent. This last figure may be taken as an average.

The efficiency of the system—viz., the ratio of work transmitted by turbine shaft to work done by motors is,

$$90 \times 89 \times 90 = 72\%$$

The length of the line actually in service is about 60 kilometres.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Month of MARCH, 1893.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

F. Uppenborn—The Electric Central Stations of Schuckert & Co.: II.—Hanover. —*E. T. Z.*, No. 9, 1893, p. 105 (I.), No. 13, 1893, p. 173.

Anon.—A New Electric Tramway in Budapest.—*E. T. Z.*, No. 9, 1893, p. 117.

Anon.—Experiments in Electric Traction at Budapest.—*Lum. El.*, vol. 47, No. 9, p. 424.

G. Richard—Electric Tram and Railways.—*Lum. El.*, vol. 47, No. 10, p. 466. —(S. I.).

G. Richard—Arc Lamps.—*Lum El.*, vol. 47, No. 11, p. 507 (S. I.).

DYNAMO AND MOTOR DESIGN.

E. Egger—Magnetic Reactions in the Construction and Use of Dynamos and Motors.—*E. T. Z.*, No. 11, 1893, p. 151.

M. Dolivo-Dobrowolsky—The Brown Alternate-Current Motor.—*E. T. Z.*, No. 13, 1893, p. 178.

G. Richard—Details of Dynamo Construction.—*Lum. El.*, vol. 47, No. 9, p. 412 (S. I.).

F. Guilbert—The Bary Motor.—*Lum. El.*, vol. 47, No. 9, p. 420 (I.).

ACCUMULATORS.

Anon.—Lighting by means of Portable Accumulators.—*E. T. Z.*, No. 11, 1893, p. 154.

J. Trumpy—Safety Devices for Accumulators.—*E. T. Z.*, No. 13, 1893, p. 177.

D. Tommasi—Multitubular Electric Accumulators.—*Jour. de Phys.*, March 2, 1893, p. 130.

Anon.—The Washburn and Michel Accumulators.—*Lum. El.*, vol. 47, No. 10, p. 474.

MAGNETISM.

H. Lehmann—The Magnetisation of Radially cut Rings of Iron.—*W. A.*, vol. 48, No. 3, p. 406 (S. I.).

E. HIRSCH—Influence of Temperature on Magnetisation.—*W. A.*, vol. 48, No. 3, p. 446 (I.).

G. ROESSLER—Researches on the Magnetisation of Iron by very Small and very Large Forces.—*E. T. Z.*, No. 9, 1893, p. 114, No. 10, 1893, p. 133 (I.) No. 11, 1893, p. 149, No. 12, 1893, p. 161 (S.).

LYDALL and POCKLINGTON—Magnetic Properties of Pure Iron. (Pro. Roy. Soc.) —*Lum. El.*, vol. 47, No. 10, p. 485.

TELEGRAPHY AND TELEPHONY.

ANON.—Communication effected between Germany and the Cameroons.—*E. T. Z.*, No. 9, 1893, p. 116.

ANON.—The Vienna Telephone System.—*E. T. Z.*, No. 10, 1893, p. 136, No. 12, 1893, p. 166.

ANON.—Telegraphy between Railway Stations and Trains in Motion.—*E. T. Z.*, No. 12, 1893, p. 166.

ANON.—The Development of Telephonic Apparatus.—*E. T. Z.*, No. 13, 1893, p. 180.

ANON.—Telegraphic Communication through the Interior of Africa.—*E. T. Z.*, No. 13, 1893, p. 180.

H. WETZER—Calling the Exchange.—*Jour. Tel.*, vol. 17, No. 3, p. 49 (I.).

ANON.—Telegraphs and Telephones in Belgium in 1891.—*Jour. Tel.*, vol. 17, No. 3, p. 53.

ANON.—Improvements introduced in the French Telegraph and Telephone Service in the Years 1891-92.—*Jour. Tel.*, vol. 17, No. 3, p. 54.

ANON.—Telegraphs and Telephones in Austria in 1891.—*Jour. Tel.*, vol. 17, No. 3, p. 59.

F. GÉRALDY—Long-Distance Telephony.—*Lum. El.*, vol. 47, No. 12, p. 562.

ANON.—Long-Distance Telephony and Preece's Law.—*Lum. El.*, vol. 47, No. 12, p. 580.

INSTRUMENTS AND MEASUREMENTS.

F. J. SMITH—High Resistances used in connection with the D'Arsonval Galvanometer.—*Phil. Mag.*, vol. 35, No. 214, p. 210.

A. ASCH—Hot-Wire Voltmeter by Hartmann and Braun.—*E. T. Z.*, No. 12, 1893, p. 162 (I.).

A. E. KENNELLY—A Differential Wattmeter for use with Alternate Currents.—*E. T. Z.*, No. 12, 1893, p. 164 (I.).

ANON.—The Willyoung Ballistic Galvanometer.—*Lum. El.*, vol. 47, No. 9, p. 425 (I.).

F. GUILBERT—Secondary Scale for Arc Lamp Photometry.—*Lum. El.*, vol. 47, No. 12, p. 573.

ANON.—The Illig Ammeter.—*Lum El.*, vol. 47, No. 12, p. 578 (I.).

ANON.—The Weston Registering Ammeter.—*Lum. El.*, vol. 47, No. 12, p. 579 (I.).

ELECTRO-CHEMISTRY.

Anon.—The Development of Dry Plates by Electricity.—*E. T. Z.*, No. 12, 1893, p. 168.

A. Ditte—The Industrial Preparation of Aluminium.—*C. R.*, vol. 116, No. 10, p. 509.

Cross and Bevan—Electrolytic Preparation of Soda and Chlorine.—*Lum. El.*, vol. 47, No. 9, p. 421.

Anon.—The Maquay Cell.—*Lum. El.*, vol. 47, No. 9, p. 422.

H. Ponthière—A New Electro-calorific Process.—*Lum. El.*, vol. 47, No. 10, p. 459.

THEORY.

H. von Helmholtz—The Electro-magnetic Theory of the Phenomena of Diffraction.—*W. A.*, vol. 48, No. 3, p. 389.

R. Malagoli—Theory of Electrolysis by Alternate Currents.—*Lum. El.*, vol. 47, No. 10, p. 451.

A. Russell—Calculation of the Impedance of Parallel Circuits.—*Lum. El.*, vol. 47, No. 11, p. 533.

P. Boucherot—Deformation of Sinusoidal Curves in Dynamos.—*Lum. El.*, vol. 47, No. 12, p. 551.

ATMOSPHERIC AND STATIC ELECTRICITY.

E. C. C. Baly—Separation and Striation of Rarefied Gases under the Influence of the Electric Discharge.—*Phil. Mag.*, vol. 35, No. 214, p. 200.

K. Ångström—Bolometric Researches on the Radiation of Rarefied Gases under the Influence of Electric Discharges.—*W. A.*, vol. 48, No. 3, p. 493 (I.).

Anon.—Earthing Lightning Conductors by means of Gas and Water Pipes.—*E. T. Z.*, No. 13, 1893, p. 182.

G. Pellissier—Action of Electricity on Water Vapour, and the Artificial Production of Rain.—*Lum El.*, vol. 47, No. 11, p. 501.

L. Palmieri—A Collector of Atmospheric Electricity giving Sparks under Quiescent Atmospheric Conditions.—*Lum. El.*, vol. 47, No. 11, p. 506 (I.).

J. Blondin—The Experiments of Lord Armstrong.—*Lum. El.*, vol. 47, No. 11, p. 513 (I.).

VARIOUS.

H. Moissan—On a New Electrical Furnace.—*Phil. Mag.*, vol. 35, No. 214, p. 313.

E. Lommel—Optical Demonstration of the Equipotential Lines in Plates carrying Electric Currents: An Explanation of the "Hall" Phenomenon.—*W. A.*, vol. 48, No. 3, p. 462 (I.).

P. Drude—On the Ratio of Specific Inductive Capacities to Indices of Refraction.—*W. A.*, vol. 48, No. 3, p. 536.

Anon.—A New Far-Distance Water-Level Indicator.—*E. T. Z.*, No. 10, 1893, p. 134 (I.).

E. L. Nichols—The Carbon Deposit in Glow Lamp Bulbs.—*E. T. Z.*, No. 11, 1893, p. 152 (I.).

E. Ducretet and E. Lejeune—Notes on the Experiments of Elihu Thomson and Tesla investigated with Apparatus devised by the Authors.—*Jour. de Phys.*, March 2, 1893, p. 126 (I.).

J. Brown—Voltaic Cells with Fused Electrolytes.—*Jour. de Phys.*, March 2, 1893, p. 131.

L. Silhol—A Method of Charging Condensers.—*C. R.*, vol. 116, No. 12, p. 621.

M. Birkeland—Electric Waves in Fine Wires.—*C. R.*, vol. 116, No. 12, p. 625.

— D'Arsonval—Influence of Frequency in the Physiological Effects of Alternate Currents.—*C. R.*, vol. 116, No. 10, p. 499, No. 12, p 630.

E. Ducretet and E. Lejeune.—Electric Crucible for Laboratory Work, with Directing Magnet.—*C. R.*, vol. 116, No. 12, p. 639 (I.).

H. Moissan and J. Violle—An Electric Furnace.—*C. R.*, vol. 116, No. 11, p. 549 (I.).

A. Blondel—Oscillograph: New Apparatus for the Study of Slow Electric Oscillations.—*C. R.*, vol. 116, No. 10, p. 502 (I.).

G. Pellissier—The Paris Fire-Alarm System.—*Lum. El.*, vol. 47, No. 9, p. 401 (I.).

L. Morisse—Orinoco India-Rubber.—*Lum. El.*, vol. 47, No. 9, p. 440; No. 10, p. 489 (S. I.).

F. Géraldy—Note on M. Cornu's Paper on "The Relation of Electrostatic to "Electro-dynamic Phenomena."—*Lum. El.*, vol. 47, No. 10, p. 464.

A. Hess—Dielectric Hysteresis and Viscosity.—*Lum. El.*, vol. 47, No. 10, p. 466.

N. Tesla—Physiological and other Effects of High-Frequency Currents.—*Lum. El.*, vol. 47, No. 11, p. 535.

A. Rigaut—Some New Electric Furnaces.—*Lum. El.*, vol. 47, No. 12, p. 575.

P. Winand—Polyphase Currents: Mechanical Demonstrations.—*Lum. El.*, vol. 47 No. 12, p. 587 (I.).

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

TABLE OF CONTENTS.—No. 106.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII. 1893. No. 106.

The Two Hundred and Fifty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 13th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 23rd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

John Henderson.	Charles Courtenay Wharton.
Henry George Shoolbred.	Frederick Young.

Mr. H. E. Mitchell and Mr. L. Leskovic were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from Messrs. Whittaker & Co., and Mr. C. Todd, C.M.G., F.R.S., Member, to whom the thanks of the meeting were duly accorded.

The following paper was then read:—

THE DISTRIBUTION OF POWER BY ALTERNATE-CURRENT MOTORS.

By Albion T. Snell, Member.

Mr. Snell. Multiphase alternate-current working, although demonstrated at the Frankfort Exhibition, has not yet found favour either in England or America; but on the Continent the Drehstrom has been used with success for several important schemes, and larger applications lie in the near future.

It has been pointed out, with justice, that multiphase alternate currents offer no real advantage in comparison with single-phase ones for merely the transmission, as distinguished from the distribution, of power. It is, perhaps, for distribution proper—*i.e.*, where a number of motors, varying in size and lying over an extended area, have to be supplied from a common and probably distant source of power—that the multiphase systems find their field of usefulness.

The object of this paper is to discuss the various alternate-current motors now in the market, and, perhaps, to see how far they lend themselves to a system of distribution in which the principal conditions of supply may be stated as follows:—

Firstly, the pressure in the transmission mains must be as high as possible.

Secondly, the pressure must be maintained fairly constant at the centre of distribution.

Thirdly, the pressure at the motor and lamp terminals must be comparatively low; and

Fourthly, the system must admit of lighting all over the area of distribution by the use of proper converters.

These points are by no means easy to obtain, even singly, and when they must be concurrent the difficulty is much enhanced.

1. Single-Phase Synchronous Alternate-Current Motors.

Single-phase synchronous alternate-current motors have been used since the memorable experiments at the North Foreland in

1884, but comparatively little progress has been made in their application to general commercial purposes; and the reason for this probably lies in the fact that the synchronous system is practically limited to a single pair of machines, for the starting of the motor by auxiliary power is not a prohibitive objection with one large machine, although out of the question with a number. On the other hand, the high efficiency of synchronous motors and their perfect regulation of speed with varying loads renders them especially suitable for a variety of purposes; and no efforts have been wanting on the part of scientists and engineers to place a self-starting single-phase motor in the market, but hitherto without success. It will serve no useful end to describe in detail all the various attempts, but the limiting conditions may be best gauged by a consideration of the difficulties involved, and by a few typical examples showing the directions in which solutions have been tried. Mr. Snell.

The following peculiarities are found in alternate-current working with single-phase synchronous generators and motors:—

(*a*) The dynamos are always separately excited.

(*b*) The motors are usually separately excited in order to save the complication of commutators.

(*c*) The motors must be raised approximately to the speed of synchronism before the dynamo circuit can be closed.

(*d*) If the load on any motor *temporarily* exceeds a certain value the machine will stop.

(*e*) The current lags behind the pressure by an angle which is a maximum when the motor is running light; and there *may* be a relatively large idle current, which, though representing little energy at the motor, causes a serious loss in the supply mains and dynamo armature. This is most marked in small machines.

(*f*) Motors must be designed not only with reference to the pressure of supply, but also with regard to the frequency of the current, and hence are only suitable for working on particular circuits; and

(*g*) Alternate motors, excited by either a direct or by a single-phase alternating current, have dead points in each period of alternation.

Mr. Snell.

The earliest attempt to meet these conditions was by laminating the field magnets of an ordinary direct-current motor and modifying the winding so as to make the self-induction as small as possible. The results were not successful. With shunt windings it is found to be practically impossible to excite the magnets excepting with large currents and few turns of wire, and even then, owing to the difference of phase between the field and armature currents, the torque is insignificant compared with the current used, and the dead points may involve difficulties. The series winding is better, but still useless for commercial requirements; for though by carefully adjusting the windings it is possible to obtain a smaller lag, the weight output is still very small and the dead points remain.

The loss by hysteresis in each case is so great as to be probably prohibitive even neglecting the other objections. The crux is the frequency. This would, most likely, have to be reduced to something of the order of 20 ∿ per second, and such a current would be useless for lighting and would largely increase the difficulty from dead points. Indeed, from this aspect the frequency should be high and the armature heavy enough to act as a fly-wheel. The author has made a number of experiments in this direction, and finds that the eye can easily detect 30 ∿ per second, and he believes from this cause it would not be expedient to work at less than from 40 to 45 ∿.

Another method involves the transformer principle, but is really only applicable to small machines. It originated with Mr. Jacobi, when one of the author's assistants, and a few motors were designed by them conjointly. With this type the armature is wound in the usual way, drum or cylinder, and has a collector of the ordinary type. The field cores are laminated and wound with two windings, one of which is coupled to the supply mains, and the other connected across the armature terminals thus forming a secondary circuit. Such a motor gives a considerable torque, is self-starting, and has probably an efficiency of 60 per cent. in 1-H.P. machines. It has dead points in each alternation undoubtedly, but they are not very apparent. The chief difficulty is that owing to the configuration of the iron of both field and

armature, and the air gap, the number of lines of force passing through the two circuits is not the same; and, moreover, there is a considerable lag between the primary pressure and current. And also, which is equally important, the primary and secondary currents are not in phase. With small armatures having few turns per coil the sparking is little, the best results being obtained when the axis of the armature field is at 90 degrees, or thereabouts, to that due to the exciting current. The armatures may advantageously be wound with two circuits so as to avoid short-circuiting coils beneath the brushes. Mr. Snell

2. Use of Condensers.

The next step in alternate motor design was the attempt to balance the self-induction of the shunt-wound field magnets by a condenser, placed in series with the windings. If the condenser be correctly chosen for the frequency and the capacity of the circuit, its E.M.F. will theoretically be equal and opposite to the counter E.M.F. of the magnets, and so the exciting current will only have to be driven against the ohmic resistance of the winding, and moreover the pressure and the field current will be in phase. The armature has but small self-induction, and so its current will be also nearly in phase with the field current, and the maximum magnetism in field and armature will occur at approximately the same instant of time. Therefore the energy absorbed by the motor can be very nearly gauged by the product of the number of volts and amperes. In practice, owing to the armature reactions, the condenser will most likely not exactly counteract the counter E.M.F. at all loads. And again, even assuming such machines to be commercially practicable, they would only be suitable for one periodic time. If condensers were applied to series-wound motors, they would be coupled in shunt to the field windings; but the objections above mentioned are equally applicable. A most important point to be noticed in connection with condensers used in this manner, is the relatively high pressure which might occur at the field terminals owing to fluctuations in the current. This difficulty can be obviated by arranging the condenser and windings in sections, but such a plan

Mr. Snell. is open to objection on the score of complication. Messrs. Hutin & Leblanc* have recently patented several modifications of designs involving condensers.

An interesting modification of the shunt-wound single-phase motor has been designed by Messrs. Stanley and Kelly. It is well recognised that the lag of the armature current does not coincide with that of the field current in an ordinary shunt-wound motor supplied with an alternating current, and hence the torque is not proportional to the armature current, but has a maximum for some particular value which must be determined experimentally for each machine. To meet this difficulty these gentlemen place a condenser in series with the shunt coils, and put short-circuited coils in recesses cut in the pole-pieces. These coils carry currents induced by, and therefore proportional to, the armature current, but always in such a direction as to tend to decrease the self-induction of the armature coils, and consequently the lag of its current also. The arrangement has the disadvantage of raising the interpolar resistance, and is likely to cause serious distortion in the main field.

It has also been proposed to supply the idle currents prevailing with alternate-current working by suitable condensers placed across the supply mains, preferably at the transformer sub-stations.

3. Split-Current Rotary-Field Motors.

Another attempt has been made in the direction of splitting the main current into two separate branches with phase differences of 90 degrees, and so producing a biphase rotary magnetic field, the armature circuit being closed on itself, or through an external resistance, and carrying a current induced by the revolving field. In principle this machine is similar to a Tesla motor supplied with two separate currents, one of them lagging 90 degrees. If a current be divided into two circuits with different coefficients of self-induction, the current in one will lag behind that in the other; and if the one circuit have no

* Under the auspices of Messrs. Rothschild, who have generously voted the sum of 175,000 francs to enable them to carry out experiments with alternate motors on a commercial basis.

self-induction but capacity, and the other have relatively a large self-induction, then the phase difference can be made as near 90 degrees as desired. Such a machine has been designed by Messrs. Hutin & Leblanc, but the author does not know how far it has been successful in practice. The points gained by this arrangement, however, mark a considerable advance in the design of alternate motors for use off ordinary supply mains. The motor is self-starting; non-synchronous; if stopped through overloading, will start again as soon as the excess of load is removed; and is self-exciting. But it appears to be still far from perfect; for, owing to the armature reactions on the field, it is difficult to see how the current in the circuit containing the condenser can be self-adjusting for varying loads with a fixed pressure of supply; and consequently the output of such a motor will probably be less than that of a Tesla one with two independent currents. The difficulties attending the condenser are precisely the same as those previously referred to with the synchronous type of motor. Mr. Snell.

Messrs. Stanley and Kelly have recently devised a quadrature motor, which presents some interesting features. In one of the circuits is placed a solenoid with low ohmic resistance, but large self-induction; and in the other circuit is a series of secondary batteries which have capacity, and, therefore, act as a condenser. The result arrived at is a lag of 45 degrees in the current of the inductive circuit, and a corresponding advance in the capacity branch, the result being a total difference of phase of about 90 degrees between the two currents. The battery does not need to have large storage capacity; and as the number of plates is arranged so as just to prevent electrolysis there will be no "forming." But lead plates used in this manner in diluted sulphuric acid rapidly sulphate, and hence are likely to give trouble. Iron plates in caustic soda are also rapidly affected, and even carbon soon crumbles away. Platinum seems to be the only reliable material, and this at its present price is too costly.

4. Multiphase Alternate Motors.

Professor Ferraris, in 1885, experimented with multiphase currents, and published his results in March, 1888, at Turin; and

Mr. Snell. in May of the same year Tesla showed a rotary motor running with four wires; and Bradley, Haselwander, Wenstrom, Elihu Thomson, Dobrowolski, Brown, and others have introduced various modifications and improvements. The first practical demonstration was made by Dobrowolski and Brown with the Frankfort Lauffen plant.

It should be noticed that in all rotary-field motors and dynamos, it is not the current, but the resulting magnetism, that rotates; thus, if there are two circuits and four poles, the magnetism will be displaced 90 degrees with each wave of current. In order to see this more fully, a simple case may be instanced. Fig. 1 is a four-pole diphase motor of elementary type, with a closed-circuit armature, having neither brushes nor commutator. There are two alternating exciting circuits, $A\ A_1$ and $B\ B_1$, in quadrature.

Suppose that A and A_1 be magnetised so as to cause lines of force to pass from A to A_1 through the armature C. Then B and B_1 will be neutral, for there will be no current in them at this moment.

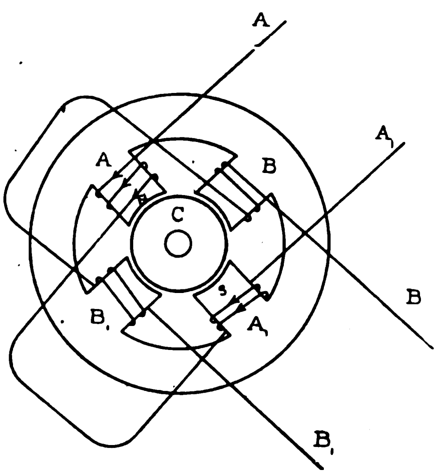


Fig. 1.—Four-pole two-phase motor.

Next, the current in $A\ A_1$ will gradually die away, and that in $B\ B_1$ will steadily rise in an equal and opposite ratio until the magnetic flux is from B to B_1, and A and A_1 are neutral.

This cycle will be repeated, but with one alteration—the current in $A\ A_1$ will rise to a negative maximum (assuming it to have been positive before), and the flux will be in the direction of A_1 to A; and in the next wave from B_1 to B. Carefully noting these changes, it is seen that the magnetic field has rotated once for a complete ∿ in each of the circuits; and therefore the speed of the field rotation is $\frac{N}{n}$, where N is the number of ∿

per second, and n is the number of pairs of poles in one circuit. Mr Snell. In the case referred to, since $n = 1$, the revolutions of the magnetic field will be the same as the number of ∿. To make a diphase dynamo it is necessary to replace the closed-circuit armature by a suitable electro-magnet having as many free poles as there are poles in one of the alternate-current circuits, to excite this magnet with a continuous current, and to cause it to revolve.*

Then diphase periodic currents will be induced in the two circuits A A_1 and B B_1, the phase difference between them being 90 degrees.

Mathematically the currents may be expressed thus:—

That in A $A_1 = K \sin a$

" " B $B_1 = K \cos a = K \sin \left(a - \frac{\pi}{2}\right)$,

which shows the phase difference between the two currents. Now K is a constant for the particular machine, and a is a measure of the angular motion of the field. It will be seen that the current in A A_1 is a maximum when a is equal to $\frac{\pi}{2}$ and $\frac{3}{2}\pi$, and is zero when a is equal to 0 and π; the converse of this is the case with the current in B B_1.

Diphase systems require either four wires—*i.e.*, two for each circuit—or else the section of the common return must be increased by 1·4 times for the same loss. This is inconvenient on account of both cost and complexity; but a far greater objection for large plants seems to be the apparently excessive variation in the field excitation, yet this view is strongly combated by many competent engineers. Let Fig. 2 be carefully examined.

---

* Rotary magnetic-field machines under certain conditions are reversible. See *Electrical Review*, February 10th, 1893, in which an experiment is given showing the reversibility of a "three-phaser." Mr. Ernst Danielson, the experimenter and writer of the article, says that this depends on the fact "that when two single or "multiphase machines are working synchronously, the current-phase is in advance of "the pressure of the generator, while it lags behind that in the motor." In practice, however, up to the present, it has been found advisable to excite the field magnets of alternate dynamos with direct currents. In the future some means may be devised by which multiphase generators may be made self-exciting; yet it is questionable whether this is worth the trouble of any complication.

Mr. Snell. Curves A and B show the current-fluctuations in the two circuits, and D that in the common return. The magnetic field

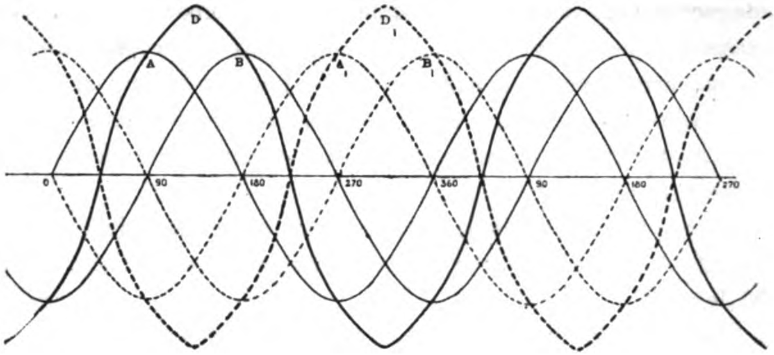


FIG. 2.—Diagram of two periodic currents with a common return. Phase difference of 90°.

will be caused by A and B alone, and, therefore, will be proportional to their sum at any instant. When A is a maximum, B is zero, and the induction at this moment is proportional to the current in A alone, and is equal to, say, C sin $\frac{\pi}{2} = C$. When A and B are equal, as at 135 degrees of field rotation, the induction is proportional to 2 C sin 135° = C 1·4. Thus the excitation varies between 1 and 1·4, the difference being nearly 30 per cent. of the highest value. The mean variation will be only about 15 per cent., it is true, but hysteresis and self-induction are proportional to the limiting values of the excitation, and eddy-currents approximately to the squares of these differences.

Self-induction decreases the output for a given weight of material, and also by increasing the lag may largely augment the idle current for small loads.

To illustrate the fluctuation of the field excitation and consequent induction the sign of the negative curves in Fig. 2 may be changed, and they may be plotted above the time line as if they were positive in value; or two fresh complete curves may be drawn, as shown in dotted lines and marked A_1 and B_1 in Fig. 2, referring to the next two impulses of current immediately succeeding waves A and B.

Both these diagrams are justified by the consideration that

each circuit in the motor contains at least two coils wound in opposite directions, and thus produces poles of different signs; and, therefore, the magnetism rotates continuously. Mr. Snell.

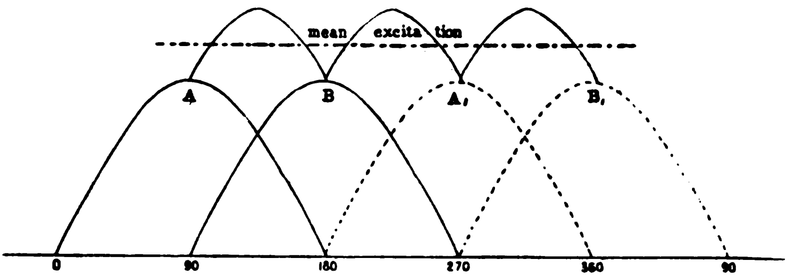


FIG. 3.—Diagram of excitation variation with two-phase current. Phase difference of 90°.

In Fig. 3 the sign of the negative curves has been changed, and the excitation fluctuation is shown by the top curves, which may be regarded as having reference to one of the poles of the rotary field, say the North pole; while with the double arrangement shown in Fig. 2, since there are two sets of curves, one positive and the other negative, the variations of both poles are indicated. But as the fluctuations are sufficiently defined by the first method, the duplication involved in the latter seems to be unnecessary.

In the preceding considerations the armature reactions have been neglected. These are not easily estimated without some experimental acquaintance with the machine in question. But their effect will be a maximum at starting, and will decrease as the armature speed increases, and would be *nil* if the speed of armature and field coincided, *i.e.*, if the motor worked synchronously. The last condition is unattainable even with no load, for the friction of the bearings and air requires a certain amount of current in the armature to overcome it. Generally speaking, the armature reactions tend to weaken the resultant magnetic field and also to lessen the torque for a given current.

5. TRIPHASE OR DREHSTROM CIRCUITS.

An inspection of Fig. 3 will show that in order to reduce the variation of the field magnetism it is presumably necessary to

Mr. Snell. increase the number of circuits. Take four with phase differences of 45 degrees. The field excitation will be nearly constant, but eight wires will probably be required; for it is doubtful whether common returns similar to a direct-current five-wire system would be practicable.

Yet, the next advance in multiphase working was made by using three circuits with currents differing by 120 degrees in phase. Since the phase difference is increased this does not seem a self-evident improvement; but on closer study it is seen that the excitation is more constant, and that the algebraic sum of the three currents is equal to zero.

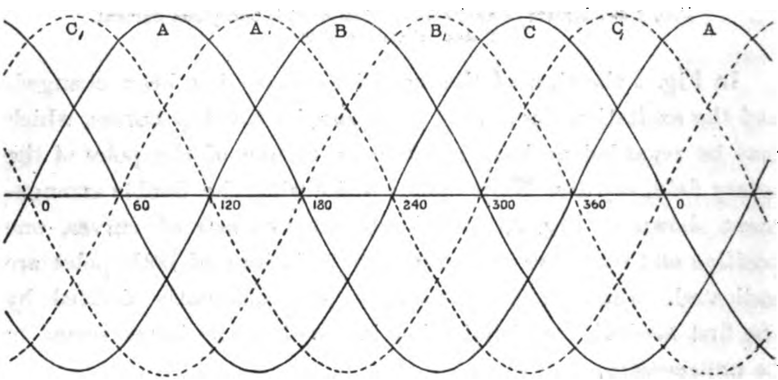


FIG. 4.—Three periodic currents separated by a phase difference of 120°.

In Fig. 4 let A, B, and C represent three periodic currents separated by a phase difference of 120 degrees; and let the instantaneous values of the currents be severally represented by C_1, C_2, C_3. Thus,

$$C_1 = K \sin a \ldots \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

$$C_2 = K \sin \left(a - \frac{2}{3}\pi\right) \quad \ldots \quad \ldots \quad (2)$$

$$C_3 = K \sin \left(a - \frac{4}{3}\pi\right) \quad \ldots \quad \ldots \quad (3)$$

And $C_1 + C_2 + C_3 = 0$ by hypothesis. For

$$K \left\{ \sin a + \sin \left(a - \frac{2}{3}\pi\right) + \sin \left(a - \frac{4}{3}\pi\right) \right\} = 0,$$

which is easily proved to be the case.

Drehstrom working is accomplished by parallel or series coupling of the circuits, the two methods being severally known

as the *star* and *triangle* systems. The author regards as the field magnet that part of the machine in which the rotary field is produced. In a triphase motor, then, the field magnet will be that part which receives the Drehstrom from the mains, regardless of whether it rotates or not. In the dynamo there can be no confusion, for the field magnets, in present designs at any rate, always rotate and are excited by a separate direct current. In this paper large capitals are used to distinguish the mains carrying the Drehstrom, and italics to denote the three windings on the field magnets of the motor, or the armature of the dynamo. Suffixes are used to mark the phase order of the circuits. It is also assumed that the effective values of the current are read by dynamometers, and that of pressure by voltmeters of the hot-wire or electrostatic type. Mr. Snell.

The *parallel* or *closed circuit*, or *triangle* coupling, is shown in Fig. 5. Let A_1, A_2, A_3 be the effective values of the several currents flowing in the mains, and a_1, a_2, a_3, the corresponding effective currents in the coils. Also, let E_1, E_2, E_3 be the effective pressures at the terminals of a_1, a_2, a_3. And assume $A_1 = A_2 = A_3$; then $a_1 = a_2 = a_3$, and $E_1 = E_2 = E_3$. And let there be no self-induction or capacity.

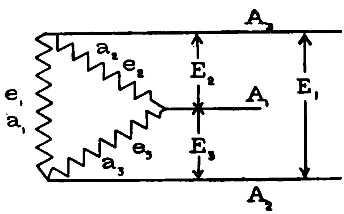


FIG. 5.—*Parallel*, or *closed circuit*, or *triangle* Drehstrom coupling.

With these assumptions, since a is in phase with e, and A lies 30 degrees removed from a; it follows, if the mains be fed with a combined three-phase current (as in Fig. 6), that the current

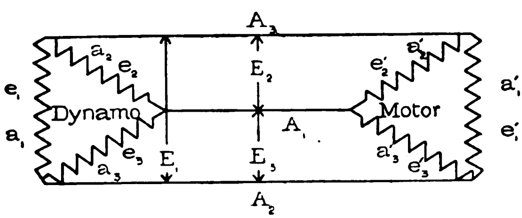


FIG. 6.—Drehstrom dynamo coupled to a motor.

in one main will differ in phase by 30 degrees from the pressure between it and the two other mains. This is seen to be the

Mr. Snell. case from the geometrical relationship of the coils and the mains. And it can also be proved that the effective value of the current in each of the mains is equal to 1·732 times the effective current in each of the coils, when the circuits are equally loaded. In Fig. 6A let the currents in the mains and in the coils

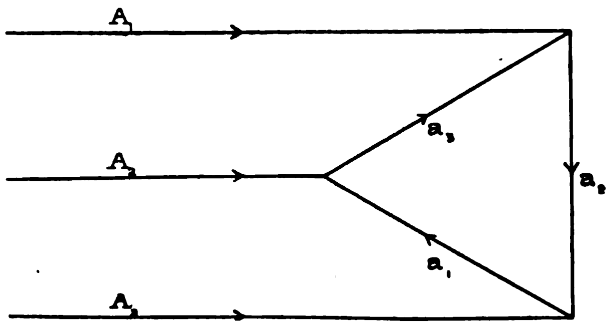


Fig. 6A.

be considered positive when flowing in the direction indicated by the arrows, and let the phase and magnitude relations of these currents be represented by Fig. 6B. Then if the diagram 6B be supposed to revolve uniformly around its centre, the length of the projections of the sides of the diagram on any straight line will represent the instantaneous values of the corresponding currents; for it is plain that these projections satisfy all the necessary conditions, viz.:—

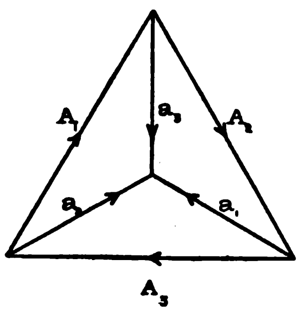


Fig. 6B.

$$\left.\begin{aligned} A_1 &= a_2 - a_3 \\ A_2 &= a_3 - a_1 \\ A_3 &= a_1 - a_2 \\ A_1 + A_2 + A_3 &= O \\ a_1 + a_2 + a_3 &= o \end{aligned}\right\}$$ All instantaneous values—*i.e.*, the lengths of the projections of the corresponding sides of Fig. 6B.

Again, the sides of the diagram 6B are proportional to the effective values of the corresponding currents; and if the load be equally distributed betwen the three mains, then for effective values,

$A_1 + A_2 + A_3 = O$; and $a_1 + a_2 + a_3 = o$; and $A_1 = 2a_2 \sin 60^\circ$

Mr. Snell

or, generally, $A = 1{\cdot}732\ a$.

The above demonstration was suggested by Dr. Sumpner.

By a similar diagram the relation between the effective pressures on the coils and mains in the *open* or *star* arrangement can be shown to be $E = 1{\cdot}732\ e$. See (*b*) of this section.

The relative position of the phases of currents and pressure in the mains and coils of a combined Drehstrom circuit of the closed type with no self-induction are shown in Fig. 7 (E and *e* have the

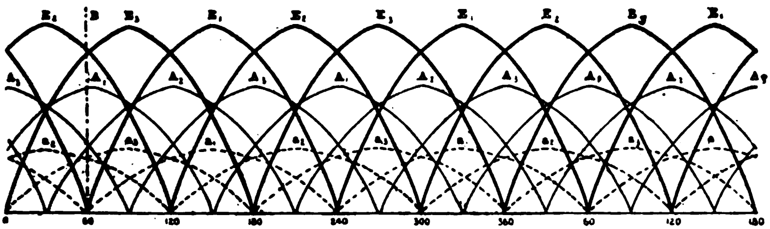


FIG 7.—Diagram of relative position of curves of currents in mains and coils of a closed-type combined Drehstrom circuit with no self-induction or capacity.

same value, and coincide in phase). It will be seen that the maxima of the currents in the mains, A, are always midway between those of the coils, *a*, and that the phase difference between them is 30 degrees.

(*b*.) The *series*, *open circuit*, or *star* coupling is shown in Fig. 8.

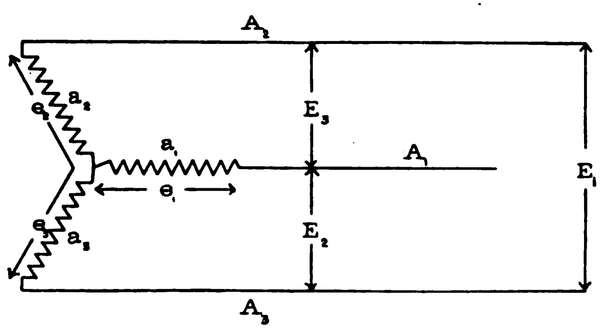


FIG. 8.—*Series*, or *open circuit*, or star Drehstrom coupling.

As before, let there be no self-induction or capacity, and let $A_1 = A_2 = A_3$; $a_1 = a_2 = a_3$; and $E_1 = E_2 = E_3$. Now, since the mains A are in series with the coils *a*, and there is by hypothesis no self-induction or capacity, $A_1 = a_1$, $A_2 = a_2$, and $A_3 = a_3$. But

Mr. Snell. the pressures between the mains are not the same as those at the coil terminals. And e will be in advance of E by 30 degrees, and will be numerically equal to $\frac{E}{1 \cdot 732}$, or $e = \frac{E}{2 \sin 60°}$, and therefore $E = 1 \cdot 732\ e$.

The phases of current and pressure in a combined Drehstrom circuit of the open type are shown in Fig. 9 (A and a have the same value, and coincide in phase).

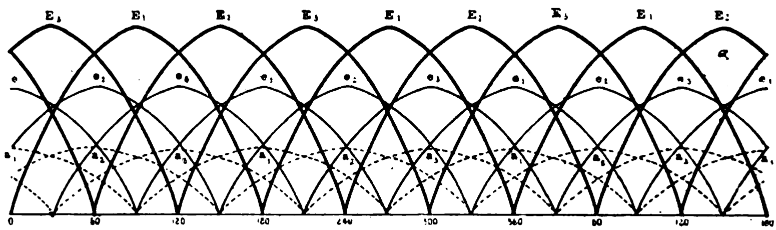


FIG. 9.—Diagram of relative position of curves of pressures and currents in the coils and mains of the *open-type* combined Drehstrom circuit with no self-induction or capacity

The conclusions thus arrived at are true only on the assumptions (a) that the coils themselves have no self-induction, and (b) that both the coils and the mains are equally loaded. The first condition is never found in practice, and the second is only likely to obtain with small motors, and then only in an approximate degree. The effect of self-induction is to cause the current in the coils to lag behind the pressure at the terminals. To measure exactly the power in a Drehstrom system, then, is a complicated task; but it can always be done by measuring the work performed in each of the separate circuits and adding the quantities.

In Fig. 5, the energy $= 3\ a\ e = 3\ E \frac{A}{1 \cdot 732} = E\ A\ 1 \cdot 732$ if the three circuits be equally loaded, and there be no self-induction or capacity.

In Fig. 8, also, the energy $= 3\ a\ e = 3\ A \frac{E}{1 \cdot 732} = E\ A\ 1 \cdot 732$ on the same assumptions.

So it appears if there be no self-induction and an equal load in each circuit that the number of amperes in one of the mains multiplied into the pressure between two mains into 1·732 gives

the power in watts. If there be self-induction, the above quantity must be multiplied by the cosine of the angle of lag between the current and the pressure. The energy absorbed by a motor, therefore, will be expressed by E A 1·732 cos ϕ, where ϕ is the angle of lag. Mr. Snell.

If the three circuits be equally loaded, the power can be measured by one wattmeter (see Fig. 9A). Put the current coil

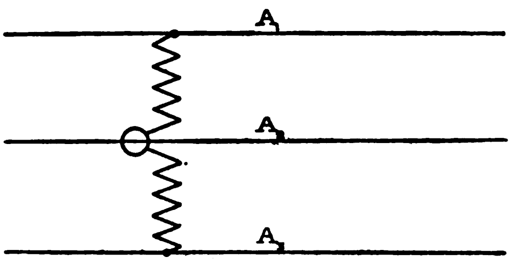


Fig. 9A.

in one of the mains, say A_2, and take two readings, one with the pressure coil coupled between A_1 and A_2, and one with it coupled between A_2 and A_3. The power will be equal to the sum of the two readings.

And if the load be unequally distributed, two wattmeters are required (see Fig. 9B). Place the current coils in two of the

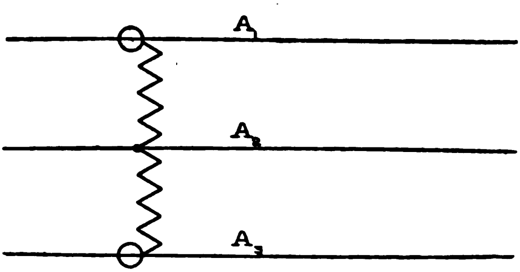


Fig. 9B.

mains, say A_1 and A_3, and couple one pressure coil between A_1 and A_2, and the other betwen A_2 and A_3. The power is then the sum of the two wattmeter readings.

The author is indebted to Dr. Sumpner for the above methods of measuring the power in multiphase circuits by wattmeters. They are also applicable for reading power with direct currents.

In part (a) of this section it was shown that with combined

Mr. Snell. three-phase currents, even when neglecting the self-induction of the coils, there is a constant phase difference between the current and the line pressure. This, although not in itself a direct loss, is the cause of great difficulty in measuring, regulating, and controlling the currents. Hence it has been found expedient to work the dynamo circuits unconnected and to use six or more separate coils. The currents from these are combined by a suitable transformer so as to convert the secondary currents into a combined high-tension rotary current with phase differences of 120 degrees. This high-pressure current is reduced at the motor end of the line by another suitable transformer and subdivided as required. In the transformers, says Mr. Dobrowolski, there is not the same constant difference of phase of 30 degrees (between the current and the pressure); but, owing to the connections of the secondary coils, the transformation ratio is not equal to that between the primary and secondary turns; so the latter must be increased to the extent of this difference. The ratio is 2 : 1·732, or a difference of 14 per cent., neglecting all losses from other causes than the connections. The efficiency of Drehstrom transformers is probably not quite so high as that of single-phase ones, but it is not much less; and when the advantages of low-pressure dynamos and motors with high-pressure transformers is fully appreciated, the slight difference in efficiency appears insignificant.

The next point for consideration is the pulsation of the magnetism in the rotary field. This is not a simple matter to represent in a graphic manner, for the magnetic field is not merely the result of the *inducing* currents in the Drehstrom coils; but it is also largely affected by the frequency and magnitude of the *induced* currents in the closed coils of the armature, which vary from instant to instant with every fluctuation of load. The frequency* of the induced currents will always be less than that of the exciting current—attaining its highest value at full load when the slip of the armature is greatest. It is expressed

* There are two periodic currents in the closed coils—one of high frequency, varying with the magnetic pulsation, and one of low frequency, varying with the speed of rotation. The torque is mainly due to the latter, which is here referred to as the frequency.

by $\frac{N - n}{60}$, where N = the number of revolutions of the magnetic field per minute, and n = that of the armature. (If N = 2,400 and n = 1,920, then the frequency will be 8 per second.) The average pressure acting in the closed coils will be given by the expression K C F a 10^{-8}, Mr. Snell.

where K = a constant,
C = some function of the winding,
F = the average number of lines of force from one pair of poles,
and a = the number of ∿ per second of the armature induced currents.

In determining the excitation for multiphase motors and dynamos, it is therefore necessary to make two calculations, one for full and one for light load, just as with direct-current machines.

It is clear that it is not possible to represent the changes of the rotary field magnetism by a general diagram. Fig. 10

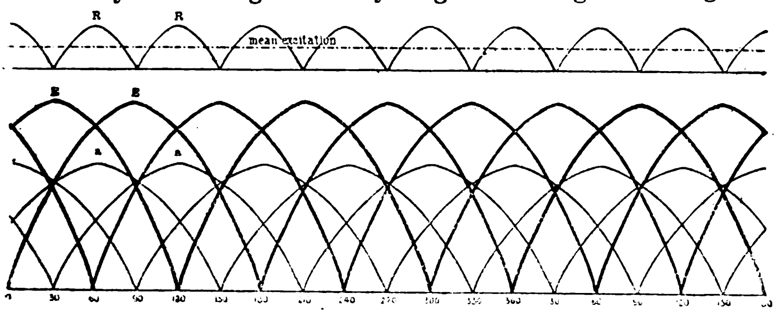


Fig. 10.—Diagram of the excitation fluctuations in a combined closed-type Drehstrom circuit.

shows diagrammatically the relative position of the current and pressure curves in a combined Drehstrom circuit, with the resultant excitation, assuming the armature reactions to have no effect—*i.e.*, the field and armature are supposed to rotate at nearly the same speed. An arbitrary angle of lag of 30 degrees between the pressure and the current in the exciting coils has been assumed, and the curves have been drawn to suit the sine law. The pressure is shown in E_1, E_2, E_3, and the current in a_1, a_2, a_3. The resultant excitation is indicated in curves R, R, R, the limits of which are 2 a and 1·732 a, a mean difference of about

Mr Snell. 7 per cent. If the magnetism varied as the excitation, then the same fluctuation would occur in the rotary field, but owing to hysteresis and eddy-currents the mean variation is probably not much more than half as great, say from 4 to 5 per cent.

Now the torque will vary with the ampere-turns, and the number of lines of force in the armature; or, the torque $= \frac{C\, i_a\, F}{K_1}$, where $i_a =$ the armature current, and $K_1 =$ a constant, and the other symbols have the same signification as above. The magnetism is determined by the field excitation, and the number of turns of wire is of course fixed for a given armature; hence, to increase the torque, it is necessary that the armature rotate more slowly.

This "slip" increases the frequency, and consequently the magnitude, of the armature current, thereby weakening the resultant field and lowering the counter E.M.F. of the field-magnet coils, and allowing more current to flow in the exciting circuit. But since the total magnetism decreases as the armature current increases, there is a point at which the torque is a maximum. This corresponds to a definite current,* which is not the starting or maximum current, and hence these motors do not exert their greatest effort at starting. In order to avoid large rushes of current when closing the exciting circuit, it is advisable to insert variable resistances in series with the armature windings. This is specially necessary with large machines, and it appears probable from this reason, as well as from other considerations, that large Drehstrom motors will always be designed with stationary armatures and revolving field magnets.

So far, if the difficulty of measuring the power be disregarded, the main point calling for improvement appears to be the magnetic pulsation. The mean variation does not, theoretically, exceed 7 per cent., and, practically, probably not 5 per cent. In

* It is clear that the particular current for maximum torque is dependent mainly on the magnitude of the armature reactions, and if the latter were sufficiently small the greatest torque might be obtained at starting, as is the case with a series-wound direct-current machine. With commutatorless Drehstrom motors it is probable, in the author's judgment, that the maximum effort will be obtained with a current little greater than that required for the greatest output.

order to still further decrease this, Dobrowolski has devised a very pretty combination of the open and closed type windings, which reduces the mean variation of the excitation to 3·5 per cent., when the magnetic field should be practically constant. The winding is shown diagrammatically in Fig. 11, and is known Mr. Snell

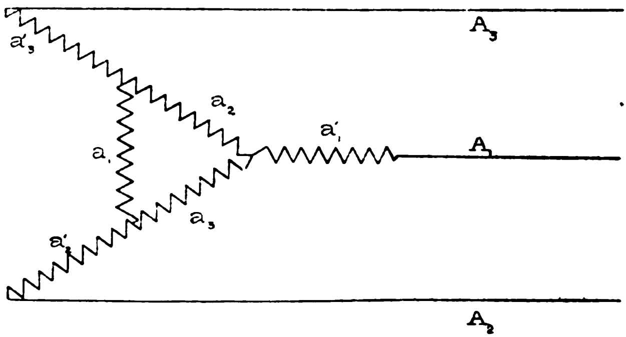


FIG. 11.—Combined *series* and *parallel* Drehstrom couplings.

as the double-linked winding. The coils indicated by the symbols a_1^1, a_2^1, a_3^1, are severally wound in two parallels, 15 degrees removed from the closed coils a_1, a_2, a_3. The complications involved are considerable, and the gain perhaps not commensurate with them; yet for large machines the device may prove useful.

There are some important points to be noticed between the *series* and *parallel* connections. It has been shown that with the *series* coupling $e = \frac{E}{1 \cdot 732}$ (see Fig. 8), and that the currents in the coils and mains are the same; and in the *parallel* device (see Fig. 5), that each coil carries a current equal to $\frac{A}{1 \cdot 732}$, and that the pressure at the coil terminals corresponds to the pressure between the mains. These differences are suggested by the terms *parallel* and *series*, which are in this respect more apt than those of *triangle* and *star*.

Now the magnetising effect exerted by a given number of turns of wire, and a definite effective current in the mains, will be different with the two kinds of windings.

Consider Figs. 12 and 13, which severally represent the *parallel* and *series* couplings, and the direction of current at the

Mr. Snell. instant when the drehstrom is entering by one main and returning equally by the two others. Let n be the number of turns of wire

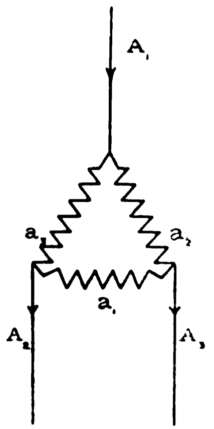


Fig. 12.

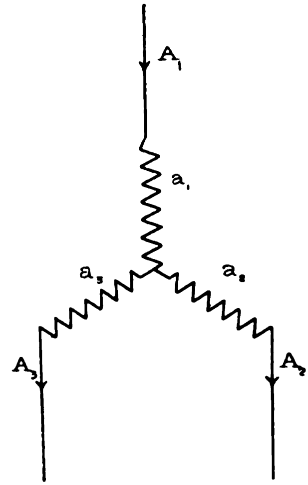


Fig. 13

in one coil. Then the total ampere-turns in the *parallel* device is n A, and in the *series* arangement $\frac{3}{2}$ n A. Therefore, the magnetising effect and also the self-induction of the *series* device is greater than that of the *parallel* for a given current and a fixed number of turns. Now, the total power absorbed by the two circuits may be made the same by a proper adjustment of pressure and frequency; but since the lag is involved, a complete solution is not very simple. Probably, however, a few experiments made with different-sized machines will do more to assist the constructor than pages of mathematical theory.

It must be borne in mind that the triphase system has not been definitely proved to be superior to the diphase, and the leading authorities on the subject differ strongly. Herr Dobrowolski says that "the diphase current now belongs to the "past," because in practice machines give curves departing so widely from sine functions. He admits, however, that if the phase differences of current and pressure be the same in each of the two circuits, and *follow a sine law*, the work is the same in each at the same instant. Messrs. Brown, Kelly, Stanley, and

Kapp all maintain that there is little to choose between the two Mr. Snell systems from this point of view; and Mr. Brown further states that "his extensive experience with multiphase currents shows that " two- or three-phase motors of similar outputs are much the same " machines with regard to their respective efficiency, starting torque, " weight, and apparent watt consumption. This holds good for " synchronous as well as non-synchronous machines." It is generally admitted that the Drehstrom presents many difficulties for lighting, and still more for a combination of lighting and power work; but Continental engineers have to a large extent successfully overcome these. Whereas, on the contrary, the diphase system admits of as simple a treatment as the single-phase one. It appears to be therefore probable that, if multiphase currents come into general use, the diphase may be employed chiefly for combined *light and power distribution;* and that the Drehstrom may find its field of usefulness in *transmission* and *distribution of power.* Experience alone can settle these points.

6. Practical Application of the Drehstrom.

With a view of adding to the somewhat scanty practical information on the subject of multiphase working, the author proposes to lay before the Institution a few tests of two Drehstrom machines, designed and built by himself some eighteen months ago. These machines were adapted from material at hand, and were built under restrictions as to cost and time, and thus can only be regarded as rough experimental designs. The dynamo was a Gramme-wound 10-unit two-pole shunt machine, designed to give 160 volts and 60 amperes, at a speed of about 1,000 revolutions per minute. Three insulated copper rings, with suitable brushes, were fitted around the commutator and connected to the windings of the armature at points 120 degrees apart. The ordinary commutator and brushes were left intact, so that both direct and triphase currents could be taken from the armature; and the field was separately excited. At a speed of 720 revolutions per minute, the frequency was 12 $\sim$ per second. This was afterwards increased to 33 $\sim$ by raising the speed of the armature.

The motor was specially built, and had a laminated ring

Mr. Snell. magnet, with Gramme windings, and a closed-circuit armature. It was estimated to give 3 B.H.P. at about 1,200 revolutions per minute, and the gross weight was approximately 300 lbs. The armature plates had holes drilled parallel to the shaft and as near to the periphery as possible without breaking through. In these were placed lengths of No. 4 S.W.G. copper wire, insulated from the plates and soldered at either end to massive copper rings. The field winding consisted of 6 coils, each containing 68 turns of No. 6 S.W.G. copper wire, and having a resistance of 0·055 of an ohm. Opposite coils were joined in series with each other, so as to make three separate circuits, each of 136 turns and 0·11 of an ohm resistance, which could be connected in star or triangle fashion, as desired. This motor proved to be a very effective machine, having considerable torque at starting, and a speed regulation quite as good as a direct-current machine of similar output. The only instruments available for taking electrical measurements were a Cardew voltmeter and a Siemens dynamometer; hence it was not possible to measure the angle of lag, or estimate the true watts. The experiments made with the dynamo and motor are shown in the following curves and tables, which at this stage may be instructive, since they show the order and direction of the difficulties met with in Drehstrom working.

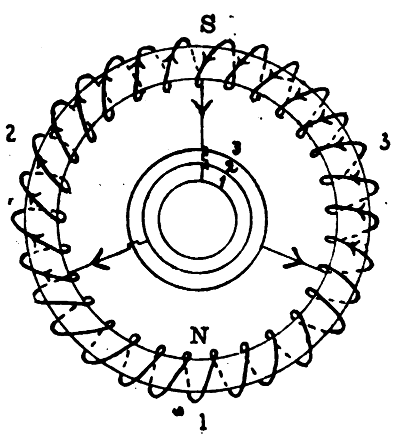


Fig. 14.—Connections of Drehstrom Dynamo coils in series.

(a.) Preliminary Experiments with the Dynamo.

The three copper rings were connected to points in the winding at 120 degrees apart, the Gramme winding being otherwise untouched. The arrangement is diagrammatically represented by Fig. 14. The field was separately excited by 3·05 amperes, and the speed was kept at 710 revolutions per minute. The load was absorbed by iron spiral

resistances with small self-induction. Readings were taken of the direct pressure; the alternating pressure at the ends of the three coils, a_1, a_2, a_3; and the current in each of the three mains, A_1, A_2, A_3 (see Fig. 15, curves 2 and 3). It was found that with the Mr. Snell

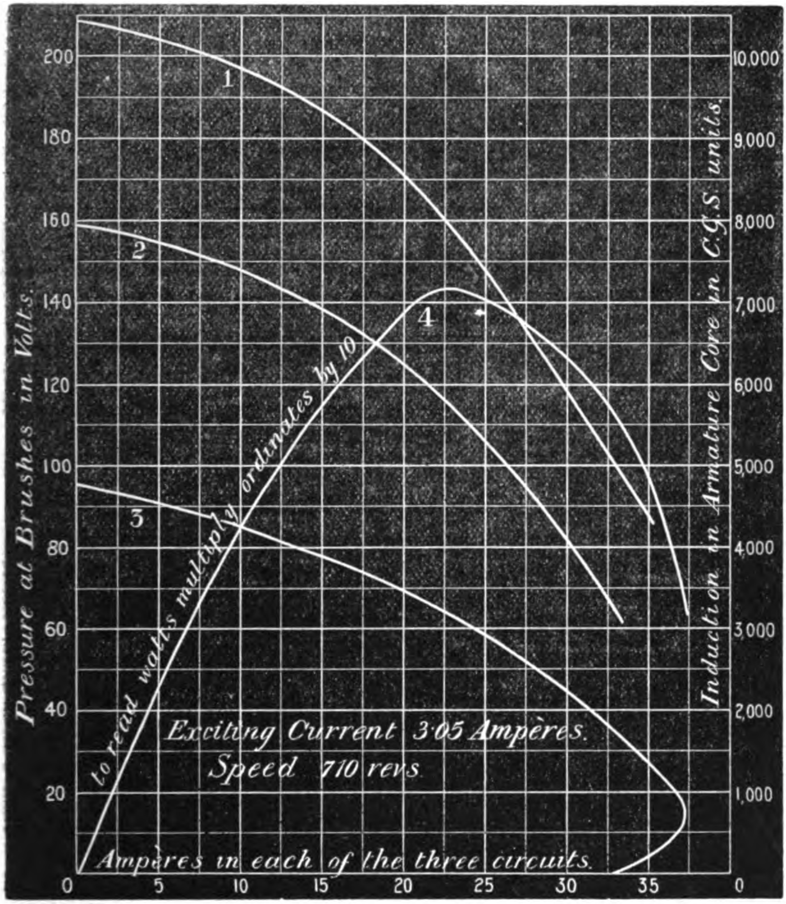


FIG. 15.—Curve 1. Induction in armature core. Curve 2. Direct pressure. Curve 3. Alternating pressure. Curve 4. Apparent watts in each circuit.

armature unloaded the pressure developed by each coil was practically the same; this was also the case when they were equally loaded (see Fig. 16). But it fell rapidly as the current increased, and so a considerable difference could be made between the pressure of the three circuits by simply giving different loads to each of them. The magnetic induction of the armature core, curve 1,

Mr Snell. Fig. 15, was calculated from the usual formula, $E_a = \frac{N A n}{10^8}$, where E_a equals the total number of volts in the armature, A the number

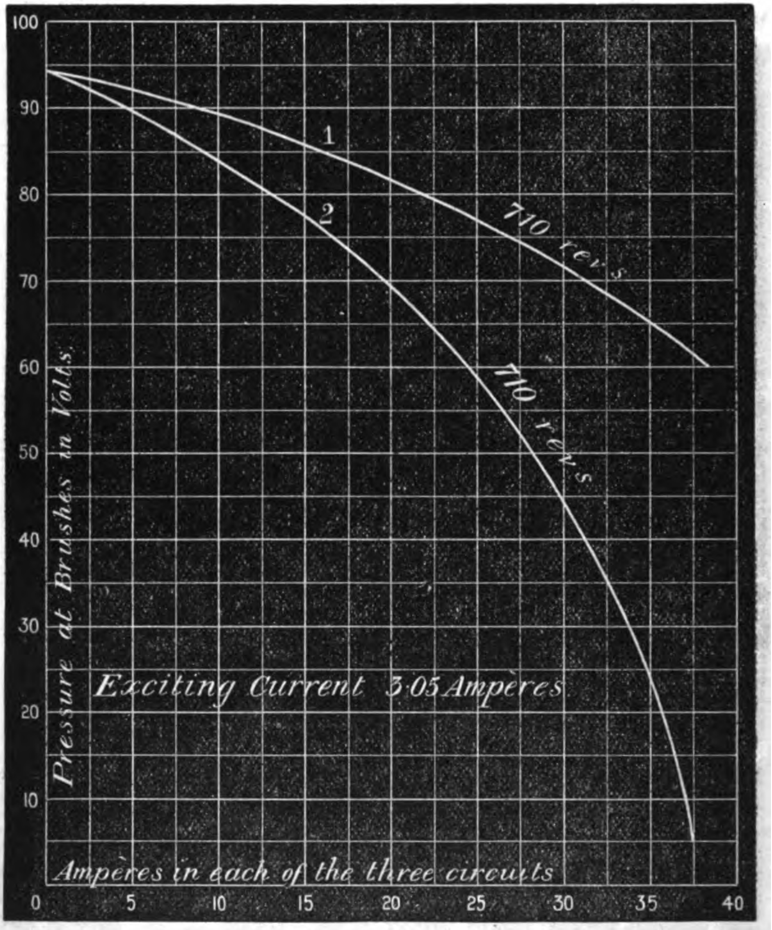


FIG. 16.—Shows fall of pressure at terminals of Drehstrom dynamo with armature coils coupled as in Fig. 14. Curve 1. Only one circuit loaded. Curve 2. All three circuits equally loaded.

of turns counted all round the armature, N the total flux, and n the number of revolutions per second. It will be seen that the values of the induction measured were all small. The frequency was 12 per second. These conditions were not the most favourable, it is true, but they showed the detrimental effects of internal resistance and self-induction in not only restricting the output, but

also in affecting the regulation of the pressure of distribution. Mr. Snell.
It will also be seen that the direct pressure is higher than that of the alternating in the proportion of 158 to 94 on open circuit; and with 20 amperes in each branch (or a total of $1 \cdot 732 \times 20 = 34 \cdot 64$ from the combined circuit) in the proportion of 124 to 70. This difference is to be expected, since the alternating pressure is the average value of 120 degrees of the differential curve of potential; and, further, when the armature is loaded, the self-induction probably increases the difference between the two values. The apparent output, curve 4, Fig. 15, estimated by the product of effective amperes and volts, is found to be a maximum with about 22·5 amperes in each circuit, or 39 in all. The current was next increased in each circuit so as to find the slope of the external characteristic curve. It was found to steadily fall at about the same slope until 37·5 amperes were reached, corresponding to a torque due to 65 amperes, this current being rather more than that designed for the armature. At this point the belt began to slip, and the experiment was stopped. It is clear that this class of dynamo can be safely overloaded for short periods.

The winding of the armature was next divided into six equal parts and opposite sections were joined in parallel (see Fig. 17).

The copper rings were then connected as before to points at 120 degrees from each other. Each of the three circuits had now only half as many turns as with the coupling shown in Fig. 14, and one-fourth the resistance. The effect of this alteration was to lower the internal resistance of the armature and also, presumably, to reduce the self-induction. The excitation was maintained as before at 3·05 amperes, and the speed also at 710 revolutions. The

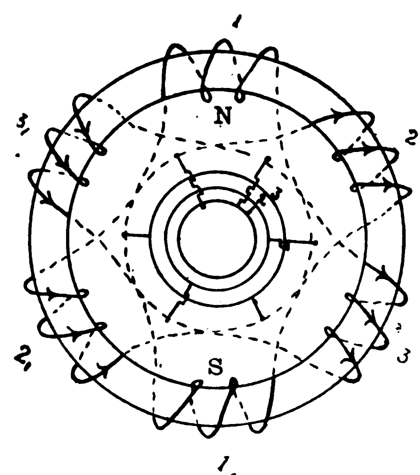


FIG. 17.—Connections of Drehstrom dynamo, opposite coils in parallel.

Mr. Snell. results obtained from the two couplings are shown in Figs. 16 and 18 respectively. It is noticeable that the parallel winding

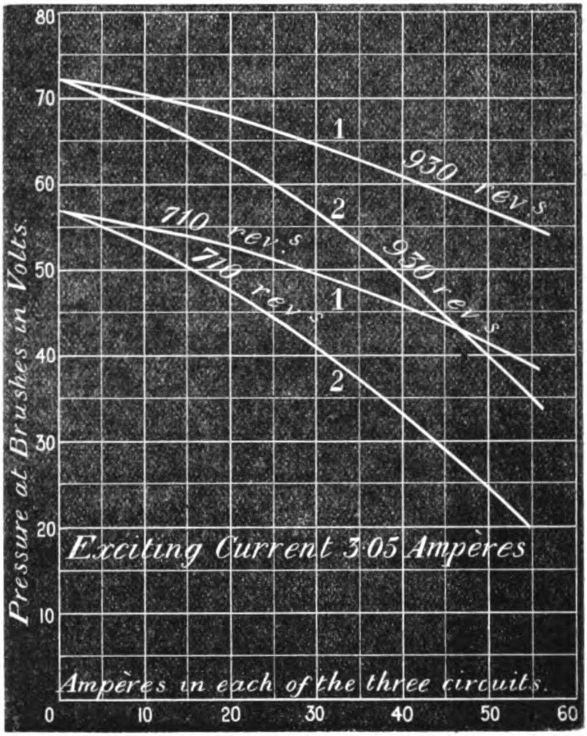


FIG. 18.—Shows fall of pressure at terminals of Drehstrom dynamo with armature coils coupled as in Fig. 17. Curves 1. Only one circuit loaded. Curves 2. All three circuits equally loaded.

has not materially affected the apparent output of the dynamo. The effective number of turns in each coil is only one-half that with the series winding; and, therefore, for the same heating effect the number of amperes is twice as great. To compare the two outputs, then, it is necessary with the series arrangement to take only half as many amperes as with the parallel one. Referring to Figs. 16 and 18, it is found that the apparent watts at 710 revolutions per minute are severally 69×20 and 34×40, or approximately the same. There was probably a smaller lag of current with the parallel than with the series connections. The smaller pressure and larger current were most suitable for testing the motor, and so the parallel connections of the dynamo armature were used during the whole of the following experiments.

(*b.*) *Experiments with the Drehstrom Motor.*

Mr. Snell.

(1.) *Star* or *open-coil* coupling. The motor was coupled to a dynamo by belting and then supplied with current at frequencies ranging between 12 and 33·3 ∿ per second. With the lowest frequency the motor ran light at about 700 revolutions per minute, being nearly synchronous with the dynamo. As the load was applied the speed decreased, and since the output was inconveniently small the dynamo speed was raised to about 900 revolutions per minute, when the following readings were taken :—

Groups A and B.

No. of Test.	Speed of Motor.	Pressure on Mains.	Amperes in each Circuit.	Exciting Current in Dynamo Field Circuit.
1	848	58	21	2·5
2	746	55	21	2·8
3	880	77	38	4·5
4	800	77	27	Not taken.
1	888	80	28·7	4·3
2	866	80	27·6	4·3
3	824	75	27·6	4·3
4	890	77	28	3·9
5	790	70	27·6	3·9
6	866	75	27·6	3·9
7	Armature held stationary	70	34·7	3·9

The frequency was about 15 in the above experiments; but the dynamo speed varied from 932 to 833 revolutions per minute, owing to the engine governing badly.

The load on the engine was next adjusted so as to give a steady dynamo speed of 960 revolutions, and the tests included in Groups C, D, and E were made.

Mr. Snell. In C a prony brake was applied to the pulley of the motor, and in D and E a dynamo was driven, as in the tests of Groups A and B, this being found to be more convenient.

Group C.

No. of Test.	Motor Speed.	Pressure on Mains.	Amperes in each Circuit.	Weight on End of 24″ Brake Arm.
1	736	84	31	2 lbs.
2	624	82	32	3 „
3	500	83	32·9	4 „
4	500	81	33	4 „
5	340	84	34·7	5 „
The frequency was 16.				

Groups D and E.

No. of Test.	Motor Speed.	Pressure on Mains.	Amperes in each Circuit.	Dynamo Speed.
1	0	79	37·8	960
2	340	79	34·8	960
3	512	78	32·5	960
4	740	77·5	30·6	960
5	820	77·5	28·15	960
6	864	76	28·1	960
1	922	91	31	952
2	914	90	32·9	956
3	0	85	39·8	970
In both D and E the frequency was approximately 16.				

Next, the dynamo speed was increased to 1,960 revolutions in

Group F, and to 2,000 revolutions in Group G; and the motor connections were altered to the *triangle* or *closed type*. This required 1·732 times the current and 1/1·732 times the pressure for the same output with the *star* or *open type* connections (see latter part of 5). The prony brake was used in Group F. Mr. Snell

Group F.

No. of Test.	Motor Speed.	Pressure on Mains.	Amperes in each Circuit.	Weight on 24" Brake Arm.	B.H.P. of Motor.
1	1,820	41·5	26·5	1	0·68
2	1,480	41	29·8	3	1·67
3	1,200	40·5	31	4	1·8
The frequency was 32·6.					

Group G.

No. of Test.	Motor Speed.	Pressure on Mains.	Amperes in each Circuit.
1	0	36·5	39·4
2	732	37	35
3	1,000	37·75	35
4	1,276	38·75	30·5
5	1,702	41·5	27
6	1,750	42·5	27 Motor running free.
The frequency was 33·3.			

Groups A and B were made to see how far the excitation of the dynamo affected the experiments. The pressure on the mains varies, of course, with the excitation, but the speed of the motor,

Mr. Snell. as far as can be seen from these rough measurements, is nearly independent of the pressure, and seems rather to vary with the frequency and perhaps the lag. The law connecting speed and current variation for constant frequency and pressure of supply is not very obvious, and evidently requires the lag to be taken into account. For example, see Group B: in test 6 the motor loaded with 27·6 amperes in each circuit and 75 volts, makes 866 revolutions; and test 7 shows zero speed for 70 volts and 34·7 amperes. Evidently the lag in 7 is far greater than that in 6. This is confirmed by test 1 in Group D, by test 3 in Group E, and test 1 in Group G. It is curious to notice how small are the variations of current between running light and full load, or even when the armature is held stationary. This large waste or idle current is most prejudicial, and if it cannot be lessened by some practical means (by condensers or otherwise), will militate seriously against multiphase motors without commutators. If the revolving part of the motor be supplied with the triphase current, these troubles will be much less; because the moving part will contain less iron, and hence the hysteresis and eddy-current losses, and the induction disturbances will be of less importance. The frequency in the induced circuit is then so small as to give little trouble.

In Group C a prony brake with a lever 2 feet long was fitted around a 6-inch pulley, and the motor was supplied with current of 16 ∿ per second. The pressure and current variations are seen to be insignificant, though the speed varies some 50 per cent. This low frequency (16) was found unsuitable for the design of the motor, since it gave too low a speed; but the tests in Groups D and E were taken so as to show the capabilities of the motor under these conditions. The power of the motor was utilised to drive a direct-current dynamo, the two machines being coupled by a belt. The figures in Group D again show large variations of speed for nearly constant pressure and small changes of current. In Group E the pressure was increased, but only a few readings were taken, as the increase in speed was insufficient to give results widely different from those obtained in the preceding tests.

In the next experiments the connections of the motor were

altered to the *triangle* or *closed* arrangement, the end in view being the increase of motor speed and consequent output. Mr. Snell.

The tests in Group F are some of the most important in the series, for the output measured by the prony brake was sufficiently large to give practical data. It will be seen that the frequency was 32·6 per second and the pressure about 41 volts. The speed again shows large variations. When running light the speed of synchronism, 1,960 revolutions, was nearly attained.

In Group G the motor was coupled to a small dynamo by a belt, and the frequency was raised to 33·3 per second. In test 6 the motor was running free, but the speed was 250 revolutions below that of synchronism. The best results seem to have been obtained with a frequency of about 32 (test 3, Group F), and it appears probable that if the arrangements had been such as to admit of a larger current at this frequency, at least 3 B.H.P. might have been taken from the motor.

These rough experiments show the importance of reducing the magnetising current to a minimum. It can be accomplished by careful attention to the design and by making the air gap as small as possible. The Drehstrom motor may be regarded as an open magnetic circuit transformer, and the only way to lessen the waste current is to make the magnetic resistance as low as possible. Mr. C. E. Brown has already suggested bedding the field as well as the armature turns in iron, the method being applicable to either ring or drum windings.

7. Drehstrom Motors with Stationary Armatures.

Triphase motors are sometimes made with the rotary magnetic field in the revolving part of the machine. This necessitates brushes and rings, it is true, and consequently simplicity of design is departed from; but it must be recollected that there can be no sparking under ordinary conditions. The brushes would probably be made of flexible strands of copper, such as are commonly used for alternators. The chief point gained is, that the most marked effects of hysteresis, eddy-currents, and self-induction are limited to the relatively small moving part of the machine; whilst the massive stationary portions, which then form the armature, are

Mr. Snell. magnetised by currents of frequency equal only to the difference between the speed of the armature and that of the rotary field. It is clear that the speed of the revolving part can never equal that of the rotary field; but at normal loads the period of the armature current will be small, and therefore the heating from hysteresis, &c., will be comparable with that of a direct-current machine, the period being probably of the order of from 8 to 10 per second. Herr Dobrowolski's 2-H.P. motor, described in the *Electrician*, August 7, 1891, is of this type, and a brief comparison with the small motor built by the author may be interesting, because it will show the order of the results obtainable from the two types of machines. Herr Dobrowolski's motor weighed approximately 365 lbs., and the maximum output deemed advisable was 4 B.H.P. at about 1,700 revolutions per minute. On open circuit it made 2,100 revolutions, and at 2 B.H.P., the rated output, about 1,950. The frequency was 35, the supply pressure 70 volts, and the torque at starting considerable. The lag was stated to be 72 degrees when the motor was running light, 28 degrees at 2 H.P., and 22 degrees at 3 H.P. The efficiency at 1 H.P. was 75 per cent., at 2 H.P., the rated output, it had reached to about 80 per cent., and at 2·3 H.P. it attained its maximum of 81·4 per cent. The author's motor weighed about 300 lbs., and at 1,200 revolutions gave 1·8 H.P. on the brake; and it was evident from the slight heating that this output could have been safely doubled if the speed had been raised to about 1,800 revolutions. But it had a larger lag than the other machine, the waste currents at small loads were much greater, and the all-round efficiency was certainly less.

8. Combined Rotary and Synchronous Motors.

A multiphase dynamo requires the revolving field magnets to be separately excited by a direct current. It is reversible and will run as a motor if supplied with a multiphase current, but is not necessarily self-starting, and probably will not start against a large torque. If, however, the magnets be rotated so as to synchronise with the rotary magnetic field, the motor will work as satisfactorily as a single-phase machine.

Attempts have been made to combine the advantages of a self-exciting rotary field and a synchronous motor. The plan proposed is some modification of the following idea:—The moving part, usually the armature, is wound with two circuits, the one closed on itself acting precisely as the closed circuits of the multiphase armature, the second supplied with a direct current and, therefore, having fixed poles. When the field is excited by a rotary current the induced currents in the closed circuits will start the armature (if the torque be not too great), and the permanent poles due to the direct current will have but little influence. But as the speed of synchronism is approached the induced currents in the armature will decrease, and the fixed poles will have more effect, until finally the armature rotation will be entirely due to them. Mr. Snell.

Such a motor has many disadvantages, besides requiring an independent direct current. It necessarily has a low efficiency at starting and at all speeds below that of synchronism; but within wide limits of load, when once started, it will run at a constant speed with a high efficiency.

The initial torque cannot be so great as that of a simple diphase or Drehstrom winding, since the closed circuits on which the starting depends are relatively smaller.

Mr. Schuckert has built a self-exciting diphase rotary-field synchronous motor on the above principles. But the method does not appear to offer any special advantages, and it is not likely that such motors will supersede ordinary single-phase alternators.

9. Single-Phase Non-Synchronous Motors—Self-Exciting and Self-Starting.

Messrs. Hutin & Leblanc, C. E. L. Brown, and others, have devised modified forms of Professor Elihu Thomson's motor, all of which will run with single-phase currents, non-synchronously and with good efficiency.

The principle seems to depend on the induction of currents in short-circuited coils, in unsymmetrical positions with reference to

Mr. Snell. the main field, but this end is not necessarily attained by brushes and rings, as in Thomson's motor,* but by the rotation of the armature itself. In its simplest form this type of motor has no commutator, and is self-exciting, but will not start. If, however, the armature be spun in *either* direction, it will rapidly acquire the speed corresponding to the torque, and will then revolve at a uniform rate.

By the combination of an additional rotary magnetic field, the armature can be made to start against a considerable torque, and the auxiliary field can be stopped after the proper speed is attained. Various devices to secure such a field are suggested by Mr. C. E. L. Brown,† the most feasible being two parallel windings on the field magnets, one with small self-induction (or even capacity), and the other with larger self-induction. These two circuits when supplied with a single-phase current will produce a rotary field, and so cause the armature to revolve. (Other forms of this ingenious motor have been suggested by Mr. Brown, for which reference should be made to the technical journals of January and March of this year.) Most of the arrangements, however, necessitate rings and brushes, and the motor is therefore only preferable to a synchronous reversed dynamo inasmuch as it is self-exciting and self-starting. These two points are of great importance with small motors, and probably the invention will be of utility in many ways.

The general details of construction are broadly similar to those of a multiphase motor, but the number of poles in the field magnets and armature must not have a common factor, and the windings of both armature and field magnets are preferably laid in grooves or holes in the iron—an arrangement which permits the magnetic resistance to be reduced to a minimum and decreases losses from Foucault currents, while the weight output is increased. This is perhaps the best type of single-phase motor up to date, but it is undoubtedly susceptible

* See *Electrician*, March 16, 1893.

† All of these have already been referred to in the first portion of this paper.

of considerable improvement, and the problem of alternate-current motors suitable for running on ordinary supply circuits cannot yet be said to be satisfactorily solved. Mr. Snell.

I have to thank several members, especially Professor W. E. Ayrton and Dr. Sumpner, for kindly discussing with me some of the points raised in the paper; but I alone am responsible for all inaccuracies and oversights.

The PRESIDENT: Gentlemen,—This subject of alternating-current motors is one of great interest at the present time. We are all looking forward to the introduction of an efficient alternating motor, and I hope this discussion will lead to some information and some novelty. I will ask Mr. Kapp to commence the discussion. The President.

Mr. KAPP: I wish to preface my remarks by congratulating Mr. Snell upon this paper, and thanking him for having brought the subject before us. If I may use a metaphor borrowed from the paper, I would say that English electrical engineers have been working with a considerable angle of lag, as far as the study of rotary-field motors is concerned. On the Continent and in America our colleagues have gone ahead at a great rate, but we have at the present time absolutely not a single alternating-current motor in practical work in this country. Some of you will perhaps say this is so because motors of this type are of no use; but, as a matter of fact, that is not the case. A good many of these alternating-current motors are doing excellent work abroad. Only three months ago I received a newspaper from the little town of Chur, in Switzerland, which was printed by motive power supplied from a single-phase alternating-current motor. It contained an illustration of the motor, and a description of the generating station and line of transmission. There is really no defect in the alternating-current motor which might serve as an excuse for our having neglected the subject so long. I hope Mr. Snell's paper will have the result of drawing the attention of English engineers to this important subject. As regards the general method of treatment adopted by the author, I would submit one criticism which applies very much to most theories Mr. Kapp.

Mr. Kapp. which have been hitherto put forth to explain multiphase current working. It is this—that authors find it extremely convenient to neglect armature reaction. If you want to investigate scientifically the theory of rotary-field motors, at the same time leaving out armature reaction, it is very much like the play of "Hamlet" with the part of Hamlet left out. Armature reaction is extremely important, and any theory which neglects it must be very imperfect. I will endeavour to show this by an example. The author has given Dobrowolski's explanation why three-phase currents are so much better than the two-phase currents, more especially when the complicated six-link arrangement is used. This is the theory when armature reaction is neglected, but practical experience does not show any marked disadvantage in the two-phase current. The reason is, that although the currents *tend* to produce a pulsating field, they do not succeed in doing so, because this field passes through the armature, which is plentifully provided with good conductors. The result of the presence of these conductors is that any tendency to pulsation is immediately checked by currents induced in the armature conductors. The power wasted by these currents is not great. It is merely that waste which is due to resistance in the conductors, and to this small extent the three-phase current has an advantage over the two-phase current. On the other hand, the ordinary two-phase current has the enormous advantage of being very easily regulated. The three-phase current has been introduced in Heilbronn. The Lauffen machines have been used, and they work admirably there. But the superintendent in charge had a heavy time of it before he was able to balance all his customers, so that the three branches should be equally loaded. The author says in a footnote that "the maximum effort will be obtained "with a current little greater than that required for the greatest "output." As far as I understand the author's argument, it seems that at starting, when the frequency of the armature current is a maximum, the torque should be greatest. As a matter of fact it is not necessarily the case. The torque may be very small owing to the useful field being more or less wiped out by armature reaction.

Mr. SNELL: That is the view I take, and I think the text of my paper makes it clear. Mr. Snell.

Mr. KAPP: In some of the diagrams, and notably in Fig. 15, the curve representing induction in the armature core run down very rapidly, and that would indicate that the particular dynamo chosen had rather a weak field magnet. Mr. Kapp.

Mr. SNELL: The reason is very simple. The field was under-excited. I had to do this on account of the E.M.F. desired.

Mr. KAPP: There is one other point. The author says that "Mr. C. E. L. Brown has already suggested bedding the field as "well as the armature turns in iron, and the same end may be "attained by perhaps a better way—*i.e.*, by winding the field coils "in drum fashion." I fail to see how that particular way of winding can affect the magnetic resistance between the surface of the field and that of the armature.

Mr. SNELL: I think we are at cross purposes chiefly on account of the definition of "field" and "armature." I now state again that I regard the "armature" as that part of the machine which has an induced current in it, and the "field" that which receives the Drehstrom current. The case referred to by Mr. Kapp is an oversight; it should be field magnets, and not armature as in the printed slips. Mr. Snell.

The PRESIDENT: I am quite sure, from the list of names before me, that there are many persons who would be glad to take part in the discussion, and they will be pleased to have the opportunity of clearing away the little fog that may have entered into their minds from certain discrepancies in the paper. Then, again, I know that the motor that has been referred to is in London, and I am sure that I express the wish of the meeting in asking Mr. Sparks to be kind enough on the next occasion to bring this novel motor of Mr. Brown's with him. We shall be all glad to see it. The President.

I have to report that the scrutineers declare the following candidates to have been duly elected:—

Foreign Member:

Charles Walckenaer.

Member:

Charles S. du Riche Preller.

Associates:

Archibald Allan Crawford.
James T. Rossiter.
Charles Bottomley Smith.
Herbert Whitney Smith.
W. P. Steinthal.

Students:

Esmond Morgan Abdel-Malek.
Ernest Bowyer.
Charles D. Burnet.
Francis William Hurt.
John Thoburn McGaw.
Thomas Whiteford.

The meeting then adjourned.

The Two Hundred and Fifty-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 27th, 1893—Mr. ALEXANDER SIEMENS, Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on April 13th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

George Edwin Fletcher. | Henry William Handcock.

From the class of Students to that of Associates—

Joseph Thompson Eliott. | Charles Arthur Gawthorp.
James Herbert Garratt. | Richard Percy Lovell.
Alfred Allen Whitlock.

Mr. H. C. Donovan and Mr. H. C. Haycraft were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from Mr. Gisbert Kapp and Mr. W. Perren Maycock, Members, to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: We have received a letter from the American Society of Civil Engineers, which I will ask the Secretary to read.

AMERICAN SOCIETY OF CIVIL ENGINEERS,
127, EAST TWENTY-THIRD STREET,
NEW YORK, *April 11th*, 1893.

F. H. WEBB, Esq., Secretary, Institution of Electrical Engineers,
28, Victoria Street, Westminster, S.W., London, England.

DEAR SIR,—In view of the probability that members of your Society will visit this city *en route* to the Columbian Exposition, the American Society of Civil Engineers has appointed Edward P. North, L. L. Buck, and Foster Cronell a Committee of Information and Courtesy at this house.

This committee will endeavour to furnish all engineers accredited by your

Society with information as to rates, localities, and engineering works of interest.

Please address such credentials and all correspondence on this subject to the Committee of Information and Courtesy, American Society of Civil Engineers, 127, East Twenty-third Street, New York City.—Yours truly,

(Signed) F. COLLINGWOOD,
Secretary.

The CHAIRMAN: It is gratifying to know that this Committee of Information has been appointed by the American Society of Engineers, and that any member of this Institution going to Chicago can, with proper credentials from our Secretary, obtain information on landing in New York as to the best method of proceeding. Those of you who are going to Chicago had better apply to our Secretary if you wish to avail yourselves of the kind services of this committee. I would also ask you to authorise me to instruct the Secretary to write a letter of thanks to the American Society of Civil Engineers for their kind and courteous invitation.

This was agreed to by acclamation.

The CHAIRMAN: We will now resume the discussion on Mr. Snell's paper, and as, in accordance with a desire expressed by our President, Mr. Sparks has been good enough to have two of Messrs. Brown, Boveri, & Co.'s motors brought here, I will first call upon him to explain them.

Mr. Sparks. Mr. SPARKS: In accordance with the President's request, I have obtained permission from Messrs. Brown, Boveri, & Co., of Baden, to bring before you to-night two motors designed by Mr. C. E. L. Brown. They are of 1 B.H.P. each, the larger being of the synchronous and the smaller of the non-synchronous type, and both are *single-phase* motors. The synchronous motor, though comparatively well known, is not described in the paper. It has no great torque on starting, and therefore requires to start up on a loose pulley. As a matter of fact, the rotating field magnets have to be placed in such a position that each brush makes contact with one segment of the field-magnet collector: then the motor will start on a loose pulley; otherwise, if the field magnets are short-circuited, each brush is touching on two segments of the collector, and you would have to move the pulley

by pulling on the belt until the brushes touched on two Mr. Sparks. independent segments. Of course, unless the motor is put in the right position, it is a trouble to start it. As a matter of fact, it is extremely easily handled, and every one, I believe, who has tried to start it has been able to do so without the slightest difficulty. The field magnets consist of four poles, each of which has a coil wound on it; the coils are connected two in parallel, and are supplied with current through a commutator.

The standing armature has four coils, connected in series, wound through perforations in the cylindrical iron stampings which surround the field magnets. The working electro-motive force is 92 at a periodicity of 40 $\sim$; I do not know of any installations working at such a low periodicity in this country. For starting the motor, the armature and field magnet are placed in parallel across a choking coil, a fixed resistance being in series with the field. When the current is switched on, the motor goes away fairly rapidly until it reaches a certain definite speed. Then part of the choking coil is cut out, the speed increases, and so on until the motor attains upwards of 90 per cent. of its synchronous speed. It then begins a sort of beat, which is easily heard, although it is not necessary to take notice of it so long as one knows that the motor is beginning to run at a fair speed. You then cut out the choking coil altogether, and at the same time connect the field magnets in parallel with a few turns of one of the armature coils. Directly you cut out the resistance in series with the field magnets, with the motor running at 90 per cent. of its synchronous speed, it, as it were, gallops up and gets into synchronism in a very remarkable manner. It is like someone going up a ladder, and suddenly you shout and make him run up very quickly just at the end. That is the only way I can describe it.

As soon as synchronism is attained you can transfer the belt from the loose to the tight pulley, and the motor can then have load thrown on it suddenly—not only its normal load, but you can put on say three times its rated load without dragging it out of synchronism; and as the commutator has a very wide neutral point you never require to shift the brushes.

Mr. Sparks. This type of motor is largely used on the Continent, and I believe has given great satisfaction, although it is unknown here in England. It runs very quietly, being easily balanced, and having ring oiling arrangements, requires practically no attention. The one shown is designed for a periodicity of 40 ∿, runs at a speed of 1,200 revolutions, and we have used it for several weeks to drive part of our factory at Charterhouse Square. Similar motors are in use abroad up to a size of 10 H.P.

The second motor is one which I think will greatly interest the Society. It is of the non-synchronous type, and is built for 40 ∿ and 120 volts. Its armature consists of sheet-iron stampings, with a number of holes drilled round the periphery, through which are threaded a number of insulated copper rods. These are all connected together by a ring on either side, so that we really have a series of short-circuited coils. There are no brushes or collectors of any kind. The field-magnet winding is similar to the armature winding of the synchronous motor; but there are two sets of windings, each of four coils, one set of coils being always in use, the second only when starting the motor. The two series of coils are so placed that one series exactly overlaps the other—that is, the winding of a coil of one series comes in the centre of the coil of the other series. The E.M.F. required by the motor shown is 120 volts, and a normal output 1 B.H.P. at a speed of about 1,200 revolutions per minute. In order to start it the current is switched straight on to the two sets of field coils connected in parallel, but in series with one set of coils is placed a condenser of extremely simple construction. It consists of a number of iron stampings, separated by wooden blocks, the intervening space being filled with a 10 per cent. solution of caustic soda. The motor on starting has considerable torque (corresponding to three times the torque at full load), and immediately runs up to a speed of synchronism, although it is a non-synchronous motor. As soon as a fair rate of speed is attained the second set of coils, with the condenser, is cut entirely out of circuit. When loaded to 1 B.H.P. it takes 11 amperes at 120 volts, or 1,320 apparent watts. When giving, during short periods, 2 B.H.P., it takes 23 amperes at 120 volts, or

2,760 watts, or gives a return in B.H.P. when thus overloaded of 54 per cent. of the apparent watts put into it. When giving 1 B.H.P. the return is 56 per cent. of the apparent watts. Now with a 10-H.P. motor of this type running at a speed of 800 revolutions per minute, weighing between 9 and 10 cwt., which is daily working in a saw-mill and giving out over 15 B.H.P., the commercial efficiency, or B.H.P., is between 88 and 90 per cent. of the real watts supplied, the apparent watts being 12 per cent. more; so that nearly 80 per cent. of the apparent watts appear on the brake. When taking maximum power from the motor exhibited (1 H.P.) the percentage of drop in speed below that of the synchronism is as a maximum 3·2 per cent., so that it is practically a constant speed motor although non-synchronous. Mr. Sparks.

The following is a comparison between the details of a 3-phase 1-H.P. motor of Herr Dobrowolski, as published by him in the *Elektrotechnische Zeitschrift*, No. 13, March 31st, 1893, and of a 1-H.P. single-phase non-synchronous motor with a periodicity of 40 ∿, as tested by Messrs. Brown, Boveri, & Co. at Baden, and designed by Mr. C. E. L. Brown; in fact, if it is not the actual motor now before you, it is a fellow one to it:—

Nominal output ...	1 H.P.	...	1 H.P.
E.M.F.	$(60 \times 1{\cdot}732) = 104$ volts		120 volts
Current (no load)	$(4{\cdot}5 \times 1{\cdot}732) = 7{\cdot}8$ amperes		6 amperes
„ (full load)	$(8 \times 1{\cdot}732) = 13{\cdot}85$	„	11 „
Apparent watts ...	1,440	...	1,320
Frequency	50	...	42
Speed (no load) ...	1,490	...	1,250
„ (full load)...	1,375	...	1,210
Percentage drop of speed	7·72 %	...	3·2 %
Weight of motor, complete	94 kilos. = 207 lbs.		64 kilos. = 141 lbs.
Temperature rise...	40°–50° C.	...	25°–30° C.

Motors of this non-synchronous type are running, or will shortly be running, in the following places:—Ragaz, Chur, Baden, Lucerne, Furstenfeld, Bruck, Cologne, Dresden, Intra,

Mr. Sparks. Rome, and Grenoble. I think we must all congratulate Mr. Brown upon the very successful results that he has obtained. The special advantages of this non-synchronous motor are the total absence of collector or commutator, the ring form of oiling, the ease of mechanical construction up to large horse-powers, the minimum of attention and repairs that such a motor necessitates, and the great torque obtained at starting. Of course many others have worked in this field, but, as was pointed out by Mr. C. E. L. Brown in his letter to the *Electrician* last month, although many have worked in a similar direction, none have perfected or put into practical use similar motors.

Mr. Swinburne. Mr. J. SWINBURNE: Like everyone else here, I am greatly interested in the paper, and in the whole subject of motors. Mr. Sparks has described a pair of motors at great length as being, he thought, new in England. This has surprised me exceedingly, because, curiously enough, the first motor he described as made by Mr. Brown, with whom my firm was working. The German, Swiss, and English patents for that motor, I may mention, are all taken out by my firm in my name. I was rather astonished, therefore, to find a motor of my own invention reflected back from Switzerland without my name even being attached to it. I am sure there is some slight slip on the part of Messrs. Brown & Boveri, or Mr. Sparks. I would like to enter a little into the difficulties that occur in that form of motor. The first difficulty in running a commutated synchronising self-starting motor is due to the sparking at the commutator. As early as 1888 I ran a motor of that sort when I was with Messrs. Crompton. I think, probably, that was the first small motor of the synchronising type ever run in this country. It was not a machine one could put into commercial use. It sparked a good deal at starting, and took a large current when it was running. Since then we have been working steadily at these motors; we have made several kinds—some that will start, some that will not start—but we have not yet made a motor which I consider we should be justified in putting into the market, though I do not think Mr. Brown's motors are any better. The form of motor produced by Mr. Brown will start, but you must remember that it is made for a

low frequency. It will not work so well when you get high frequencies, and its idle current will be even greater, as the difficulties which have troubled us and everybody else come in to a greater degree as soon as you use high frequencies. I do not think it is very much use working out a motor for a frequency of 40 for use in this country. You must start with 100, and if you want to develop your motor largely you must make it so that it will work on 133 for America. There is a very great difference between 40 and 133, as anyone who has experimented much with motors will know. The chief difficulty, as far as I know at present, is not the difficulty of starting—although, of course, that is a serious one—but the difficulty of getting motors to run without taking a fairly large current at no load. These motors, as you see by Mr. Sparks's table, take 7·8 amperes at no load, and 13·85 at full load. It must be remembered that this current has to go through a transformer, and also through the meter, and it would be a difficult thing to place a motor that takes half its maximum current at no load. Such motors may do very well for Switzerland, where there is water power and the installation is chiefly for motors; but I do not think, unless an alternating will run under nearly the same conditions as a direct-current motor, it is much use putting it on the market in this country. We can make motors with large idle currents, but do not consider them a solution of the problem. You can bring such motors forward and get accounts of them in the papers, and make a fuss generally, but I do not think they are a practical solution of the difficulty. Mr. Swinburne.

I should like to refer to the other motor mentioned by Mr. Sparks. I think we ought to give credit to whom credit is due in the case of that motor. It was, I think, invented by Messrs. Hutin & Le Blanc, in France. M. Le Blanc in particular has been working at the subject for a long time. Messrs. Elihu Thomson, Tesla, Ganz, and others have been on the same track. What M. Le Blanc did was practically to take a two-phase motor and put a condenser into one circuit, so as to get a difference of a quarter of a period, and he thus used a double-current motor with a single-current supply. There will be some difficulty about the condenser: it is troublesome; people do not want to have

Mr. Swinburne.

condensers in their houses if they can help it. The condenser also means high volts, unless you subdivide the field and put a portion of the condenser, then a portion of field, and then more condenser, and so on—a method, by the way, which I may also claim that I was the first to indicate; still, I do not think it is very practical. Then when all these difficulties have been surmounted you have on the active side of your motor the large no-load current. To get over that you must put in another condenser, or put up with the large current. I do not think, therefore, we can yet consider that the alternating-motor problem has been solved, and I do not think we ever will solve the alternating-motor problem until we make a machine which takes a reasonable idle current.

I would like to refer to the supposed advantages of the double and triple current. I think there has been a great deal too much said in favour of the multiple current. The engineer who runs alternating stations asks the inventor for an alternating-current motor. The inventor brings forward something which he says is an alternating-current motor, but it is at most a sort of play upon words. It is not an alternating-current motor at all in the sense of the engineer. He has a circuit with an alternating current, and he wants a motor to run on that circuit, and not a motor for two or three different circuits that he has not got; yet that is practically what has been given us. If you are going to run special circuits, there is no reason why you should not run direct-current circuits. Of course I am quite aware that the double and triple currents have some advantages over the single current in the ease of starting small motors, and they have an advantage over the direct current in ease of transmission from place to place at high pressures. It seems to me there is a tendency to confuse two things. If you are going to transmit large powers long distances, you can do so quite as well with a single as with a double current. The only advantage of the double current is in starting small motors. You can therefore transmit power over large distances perfectly well with the single current, but, if your installation consists entirely of motors, which is very unlikely, it might pay you to use these double- or triple-current motors. If

the installation consists of lamps and so on, as usual, and if motors are absolutely necessary too, which is also improbable, it would probably be best to transmit the single alternating current over a long distance and convert it into direct current at the end and be done with it, not to run a complicated system in which no one can measure the power or anything else, and which there is great difficulty in balancing. Mr. Swinburne.

I would now come to a question half theory and half nomenclature, and that is, the question which is the armature and which is the field magnet in this case. Ever since the question came up I have been in a minority of one as to which is the field magnet and which the armature. I do not know how orthodox people explain the matter. I will suggest a puzzle for them to solve. Take a pair of double-current machines, such as the Gramme alternators used for Jablochkoff lamps. Excite the fields of both by means of direct currents and run them in parallel. The machines A and B are then dynamos with stationary armatures and rotating internal field magnets. The stators are armatures, and the rotors are field magnets. Remove the exciter from B and short-circuit its rotor coils. Is the rotor of B now the armature or the field? It is still working as a dynamo, of course. Now throw the belt off A so that it runs as a motor. We now have a dynamo with no excitation on the rotor, and a separately excited motor: which is the armature in each case? Now put the belt back on A and remove it from B. B is then an ordinary double-current motor, and, presumably, the rotor is now the armature. To make these ordinary machines, of an old and well-known type, fit the modern nomenclature, a sort of general post as to the armatures and field magnets must take place when variations as to the conditions of running are made. I would therefore propose that there should be no departure from the ordinary practice of electricians in the case of multiple currents, and that the part which gives power with alternating currents to the mains in a generator, or takes it in a motor, should be called the armature, and the other part the field magnets. In all dynamos the armature has alternating currents, and generally the field has approximately constant induction. This constancy of the induc-

Mr. Swinburne. tion shows that the rotors in the above-mentioned machines are field magnets whether they are excited by armature reactions alone or by direct currents. In non-synchronous motors there is some slip, so that the field-magnet induction moves relatively to the field iron, but that does not in the least affect the broad distinction between the field magnets and armature of a machine. In the paper there are references to armature reaction, but generally the armature reaction is taken backwards. There is a common mistake that is worth correcting. If you take a machine in which what I call the field is notched, you get what is called a synchronous motor. If there is any unevenness in the field, it is said to be corrected by the induced alternating current, and one speaker referred to the closed copper coils on the fields as being like the secondary of a transformer. The alternating current induced in the coil even when short-circuited is not an alternating current of the same frequency as the current in what I call the armature. If it is a triple-current machine, the frequency of the alternating current is six times that of the others, and if a double machine, it is four times. That is rather an important point. If, instead of using a notched field, we make it smooth, we get a motor that slips. Then the same portion is still the field, and two currents are induced in it. One current has very slow frequency indeed; this is the current that opposes slip. It is induced by the armature reaction. Suppose the machine has a frequency of 50 and a slip of 6 per cent. The frequency of the one component will come out at three periods a second, and the frequency of the other will be 282. It is rather important to keep these things in mind, for they are a little difficult to follow; but if you want to understand them, I am sure it is necessary to go into all these points and get as clear an idea of what is going on, on the machines as possible.

Before sitting down, I would like to refer to another matter in connection with this subject. Take the Gramme machine B, already mentioned, running as a dynamo or motor with no separate field excitation. The power, according to Dr. Hopkinson's equations, is always E times, an expression involving various quantities, E being the electro-motive force that the machine will

give with no load. This machine, which has no field excitation whatever, will give no electro-motive force on no load, so that, according to Dr. Hopkinson's theory, neither the multiple- nor the single-current motor without field excitation can work. I therefore look upon the multiple-current machine as a standing monument of the inaccuracy of the self-induction theory of dynamos. Mr. Swinburne.

Mr. W. M. MORDEY: In his paper Mr. Snell makes some remarks on the subject of single-phase synchronous alternate-current motors that are, I think, open to criticism. I and others have advocated the use of alternate-current synchronous motors because for large work they have some advantages; and I think that, whatever developments may take place in alternate-current power transmission, it will be found that for large powers synchronous motors will have advantages. Mr. Snell objects to them for some curious reasons. In one paragraph he says: "The torque on any motor "must not exceed a certain value, or the machine will stop." That objection, surely, is common to all motors. In the next paragraph he says that "there may be a relatively large idle current which, "though representing little energy at the motor, causes a serious "loss in the supply mains and dynamo armature." As Mr. Snell has been experimenting, it is to be regretted he has not given us his results to show what that serious loss is. In a paper I read here a few weeks ago I gave some figures showing that the "power-factor," which is what we want to ascertain, is very nearly as high with synchronous motors as with good closed-circuit transformers. The idle current, therefore, is not necessarily serious. That motors must be designed not only with reference to pressure, but also with regard to frequency of current, is not a very serious matter. Nor is the objection that synchronous motors have dead points in each period. The dead points are very little ones! There are 200 of them in a second. I think the fact that steam engines have dead points has not been found in their case an insuperable objection, nor should it be in the case of synchronous motors. I am very much interested indeed in the motor that we have seen this evening, and in the comparison that Mr. Sparks has been able to give us Mr. Mordey

Mr. Mordey. as to the performance of two motors, one a three-phase and the other a single-phase. We have been told that we in England have been very backward in adopting multiphase working. Instead of blaming them for want of faith or enterprise, I wish to make a claim for the wisdom of those who have stood back and have refrained from introducing multiphase systems. What we have seen here to-night is the first step in the justification of that attitude. I think, as a matter of fact, that English electrical engineers have regarded multiphase working as merely a makeshift, and they have simply sat still and waited for the development of motors that would run on the ordinary lighting circuits. I agree with Mr. Swinburne that until motors are produced that can be run on the ordinary alternate-current mains—the same mains that are used for lighting—motors will be very little used except in special cases. I think that the difficulty with regard to periodicity is only a temporary one. We have had two- or three-phase motors as a makeshift. Now we are getting single-phase motors that will run up to 40 or 50 periods, and in a little while we shall have motors that will run up to 100 or 130 periods. We have had two motors from the same manufacturer, the one a multiphase and the other a single-phase, and in the comparative record given the single-phase motor appears to be the better one. And is it not a fact that for all ordinary work the most experienced people on the Continent and in America—even those who have been its main advocates—are giving up, or are not extending the use of multiphase working? We know that in the case of Frankfort, Von Müller & Lindley, after fully examining all systems—not only direct-current as against alternate-current, but the various developments of each—recommended simple alternate currents, even for power transmission. This from men having a wide experience of multiphase methods is most significant. This demand for motors on alternate-current mains is one that we are constantly coming across; but we are not coming across it, I am afraid, because of any real demand for motors; we are coming across it for what I may call electioneering reasons. I am afraid the explanation is to be found in the competition between the direct- and alternate-current methods—that commercial agents, in pushing their wares,

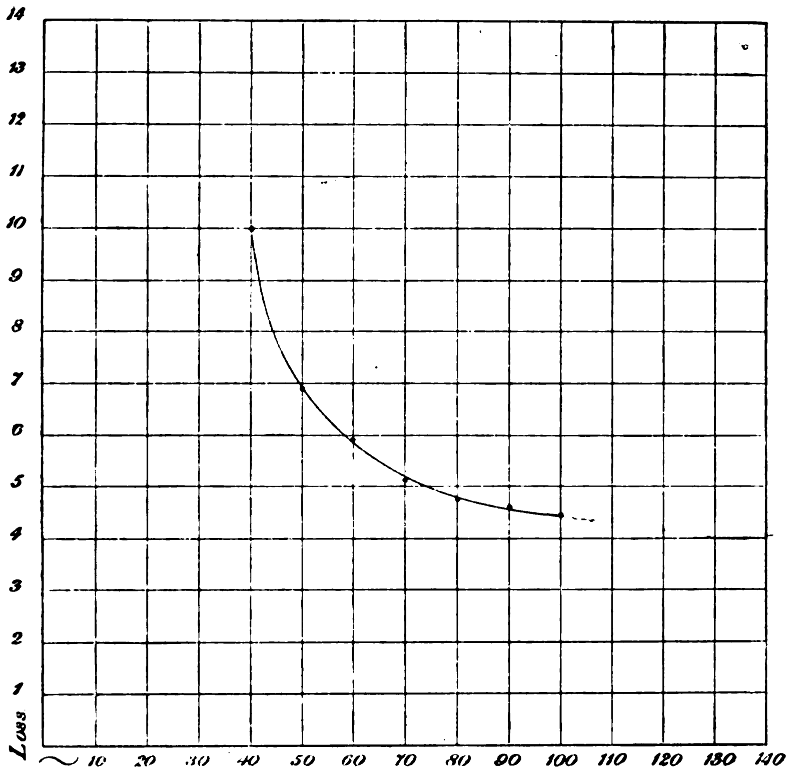
and attempting to introduce low-tension or direct-current work, in many cases make great use of an argument based on the supposition that motors cannot be used on alternate-current circuits; and then the people who are thinking of putting down installations come and say, "You cannot use motors on your circuits." If they were to examine into the output of the direct-current stations, they would not find that any appreciable proportion of the output was for motors, even where motors could be used most freely and without any difficulty whatever; and whatever alternate-current motors we may have, I do not think that on ordinary installations there will be any great use for them until the public has been educated up to a higher standard. There is not at present, I regret to say, any substantial demand for motors of any sort. Mr. Mordey.

Now I wish to say a few words about periodicity, which I consider the most important question calling for a decision at the present time. I have tried to show that we in England have done wisely in refraining from introducing one alien alternate-current development, and I wish to urge that a similar reception be given to another Continental visitor. There is a disposition to go down to 40 or 50 periods per second, instead of the 80, 100, or 130 that is being used generally for lighting transmission. One of the principal reasons why 40 periods is used so largely on the Continent may, I believe, be traced to the difficulty of designing machines of certain types to work at a greater rate. The alternators that have most largely been made for 40 periods have not, in my opinion, been so made for the purpose of using motors, but simply because they are machines that present considerable electrical and structural difficulties if any attempt is made to use them at or design them for high rates. That is one reason why these low rates have been employed, and are now being advocated. In alternate-current distribution, I think we all know that the key to economy and to good commercial results is to be found in the transformers. But to make a transformer equally good and efficient at 40 or 50 periods as at 100 is very difficult. It becomes very costly. In fact, I doubt whether anything like so good a result can, by any expenditure, be got at

Mr. Mordey. 40 periods as at 100. The transformers are either very costly, or they are wasteful, or both. I have been told that it is a question of design—that transformers can be, and should be, made specially for the periodicity at which they have to work. No doubt that is true to a certain extent, but I would point out that a transformer, whether made for 40 periods or not, will work better on a 100-period than on a 40-period circuit. My own attempts to design good transformers for 40 periods have not met with success. But—what is more to the point—I have taken 40-period transformers made by the most experienced makers, and have run them at 100 periods at their ordinary electro-motive force, and found they were very much more efficient, very much cooler, and better altogether if worked at 100 than at their own makers' rate of 40 periods. Then I have taken 100-period transformers and worked them on 40-period mains, and found they ran hot at once, and would burn out if run continuously. That, I think, shows that there is no basis of truth in the argument that you can design transformers equally well for any periodicity. And it shows that there is no truth in the argument that has been advanced that transformers do not suffer by a reduction of periodicity because of the smaller number of reversals allowing of higher magnetisation. If you take any ordinary transformer and take the magnetising current and power at different periodicities, you will find the explanation. In this curve, which I have taken from a 40-$\sim$ transformer of one of the best-known Continental makers, it is clearly shown. It speaks for itself. The loss at 40 $\sim$ is very heavy, falling rapidly as the periodicity is raised, and showing that there is not much advantage in going beyond 100 $\sim$, as, however much further you go on, you will hardly decrease the current. But from 40 to 80 or 100 you will decrease it very much, and not only decrease the current, but decrease the power also: the power-factor does not appear to alter appreciably as you go down in periodicity. This is a very interesting subject, and I wish time allowed of its fuller consideration.

In some cases the mistake has been made of using 40-$\sim$ plant, and it may be of interest if I give an example of one way of partly

avoiding the losses which was adopted in a case where my company were concerned with a central station so equipped, the Mr Mordey



transformers having been especially made for the same rate. The loss during the time of small load was of course considerable. But by the simple expedient of using during the day a 100-∿ alternator large enough for the day load we were able greatly to lessen the coal consumption, the 40-∿ plant being put on only for the heavy load. The difference in the power required can be seen in this curve so far as the transformers were concerned, and this does not represent the only saving.

We must not, of course, forget that other considerations than transformer efficiency govern this question, the most important one outside of the power-house being the surface effect in the conductors; but, fortunately, that does not become of serious amount until we exceed 100 ∿. For the reason that most of our work is

Mr. Mordey. lighting work, and that efficiency is of the utmost importance, I want to ask those who are considering the subject to suspend their judgment before going in for what I consider the fatal mistake of low-period working. The argument that is being used is that motors can more easily be used at low periods. Even if this were true at the moment—and even this can scarcely be said—we should remember that the motor difficulty is only a temporary one. We have seen it at least half solved. If we wait a little while we shall see it solved altogether. It is not nearly so important as efficiency. The importance of the question of efficiency is not properly realised. It is thought that a few watts more or less do not matter very much in a transformer; but if you reckon out the cost of magnetising transformers, and take the very lowest estimate of the power—take one penny per H.P.-hour—you will find that even at this low rate a watt continually expended costs one shilling a year, and there is probably no central station in this country that will produce a watt continually for a shilling a year in that way. You may take it that two or three shillings a year is thrown away on every watt wasted, and if you work at 40 periods you must throw away more money in this way or spend a great deal more money on transformers to start with. There is no sufficient evidence to justify us in supposing that alternate-current motors will be more efficient and better at low periodicities. There is very little difference in principle between an alternate-current transformer and a motor. One of them transforms electrical energy into mechanical energy, and the other transforms electrical energy into electrical energy. The processes are pretty much the same. If you take a direct-current transformer, you can put electrical energy in at one end, and you may either take power out electrically from a pair of terminals or put a belt on and take the power off mechanically. The transformation is either from electrical to electrical or from electrical to mechanical. It is the same thing in first principles, and I think it will be found that in motors, as in transformers, a fairly high periodicity will not be attended by disadvantages. I am reminded of a remark made by Mr. Brush a little while ago to an eminent

member of this Institution who was explaining to him the advantages of using 20 or 25 periods per second for some scheme that he was considering. Mr. Brush said, "That is right; get "a little bit lower, and you will have a good honest direct "current." I think when you get down to 20, 30, or 40 periods a second, you may in many cases just as well have a good honest direct current, because alternate currents of low periodicity have very little that can be said for them in comparison with the direct currents. Or, to put it in another way, the distance at which it becomes economical to substitute alternate current for direct current is greater with low than with fairly high periodicity. As in the case of multiphase working for general distribution, so with these low-period proposals: there is at least a probability that those who have advocated them will find, almost before they get them to work, that they are obsolete. Mr. Mordey.

Mr. H. W. KOLLE: Allow me to supplement the remarks of Mr. Sparks by two observations which I think will give some pleasure to the gentlemen who have been criticising him. Mr. Brown has already working at Chur, in Switzerland, a 3-H.P. single-phase self-starting non-synchronous motor running at 70 cycles; and we have also at the Ferranti works at present a single-phase self-starting synchronous motor which has been working on the London Electric Company's circuit of 85 cycles. Of course one great advantage in Brown's motors is the very small air gap; this Mr. Sparks did not remark upon. Mr. Kolle.

Professor AYRTON: The principle of the Brown synchronising motor described by Mr. Sparks this evening is older than I think is generally imagined. As far as I am aware, Professor Forbes was the first to publish the idea of constructing a motor in which you commutated the current round the field, producing sparking, of course, until the motor attained the synchronising speed, after which the current round the field became a simple pulsating direct current. That must have been about 1882 or 1883, for I remember about 1884 or 1885 showing such a motor running at one of my lectures at the Central Institution a year or two after Professor Forbes had given an account of the principle at a meeting of the Royal Society of Edinburgh. Professor Ayrton.

Professor Ayrton.

With reference to the names "field" and "armature," the suggestion that Mr. Swinburne has to-night put forth is exactly the one Professor Perry and I made in our 1883 paper on motors, so much abused by Mr. Swinburne. There we clearly stated that our motor had a stationary armature and a rotating field magnet, and we said that we defined the field to be that part of a motor or dynamo which, whether rotating or not, had a steady current flowing round it, and the armature as the part, whether rotating or not, which had a varying current flowing round it. When, however, you come to the Drehstrom motor, the definition is not quite so simple, since in both the stationary and the rotating portions the current alternates.

Mr. SWINBURNE: The induction remains constant in the field.

Professor AYRTON: The induction is varied in the armature.

Mr. SWINBURNE: Yes.

Professor AYRTON: The induction in both is varied. With the Drehstrom motor there is always a certain amount of slip when there is a load.

Mr. SWINBURNE: It varies 5 per cent.

Professor AYRTON: Then you have a varying current and induction in both parts; therefore the only way to improve the definition is to say that in a machine consisting of two parts with a varying current round each we call the field the one in which it varies less, and the armature the part in which it varies more. You cannot use the old definition suggested in 1883 by Professor Perry and myself that you should call the part, whether rotating or not, round which the current does not vary at all, the field. In the Drehstrom motor with a load there must be slip, therefore you have a varying current and induction in both the stationary and the rotating portions, although it varies much more rapidly in one part than in the other; but you may call the part in which the current varies most rapidly the armature, and the one in which it varies less rapidly the field, and this I suggest as the most logical definition.

As to the question of motors affecting the choice between what Mr. Mordey has called the honest direct current and what I suppose he would go on to call the dishonest alternating current,

I certainly agree with him that in this country at the present time it ought not to affect the choice at all, because, as a matter of fact, as he said—and it is important to grasp it fully—motors are so little used, at any rate in residential towns, that it is of little consequence indeed whether or not the alternate-current motor is less or more efficient than the direct-current motor. In advising a scattered residential town a little while ago to use the alternating current, I told the authorities that the present inferiority of the alternating-current motor was not worth while considering, and if they had any difficulty about the matter, and anybody wanted to use alternate-current motors and objected to their inefficiency, it would pay the authorities to supply power at 2d. or 3d. per unit, while they were charging other people 8d. or 10d. per unit for electric power for lighting. The advantage of employing transformers for the distribution of the electric light current is so great in scattered towns that the small disadvantage of inefficiency in the alternate-current motors is not really worth considering. Professor Ayrton

Coming now to the paper itself, there is one suggestion that I might make. Mr. Snell speaks of so many alternations per second. I do not think that that is a particularly good expression. He means, I think, so many periods per second. Of course, in one period there are two alternations. It is rather liable to lead to confusion if you speak of alternations and you mean periods; that is for every one thing you mean really two.

Mr. Kapp pointed out last time why the theoretical variation in the magnetisation of the field, as we may call it, of a Drehstrom motor is too great,—why theoretical considerations make the variations greater than they really are. He pointed out that one cause was the reaction of the armature. Of course another cause is that these calculations apply merely to the variation in the exciting current. For we know that when iron is magnetised the induction does not vary nearly as much as the magnetising current; therefore this calculation, which makes 14 to 20 per cent. and so on variation in the excitation, does not correspond with anything like the same amount of variation in the magnetism, which is the important thing, of

Professor Ayrton.

course. In speaking of the series or open-circuit winding, Mr. Snell speaks of the potential difference of the terminals of the coil lagging 30 degrees behind the potential difference between the mains. That is a mistake. It should be the potential difference between the terminals of a coil in this series winding is 30 degrees in advance of the potential difference between the mains. He goes on to say: "It was shown that with "combined three-phase currents, even when neglecting the self-"induction of the coils, there is a constant phase difference between "the current and the line pressure. This, although not in itself "a direct loss, is the cause of great difficulty in measuring, "regulating, and controlling the currents, and also reduces the "output for a given weight of material." He seems to conclude that because there is a phase difference between the current and the line pressure, this reduces the output for a given weight of material. The argument seems to me to be defective, and for this reason—the difference of phase which he is speaking about is not a difference of phase between a current in a wire and the potential difference at the terminals of that wire, but a difference of phase between the current in one wire and the potential difference at the terminals of another wire; or, to put it more simply, in this three-phase system, if you have no self-induction, there is no difference of phase in any part of the system between the current in a wire and the difference of potentials which is producing the current in that wire; so that, in fact, his reasoning on this point falls to the ground. But beyond that, as a matter of fact, it is not true that the output for a given weight of material is less with the three-phase system than with the two-phase. As was pointed out some years ago by Dobrowolski, with the three-phase system of transmission, with three wires on your poles, each wire carrying a certain maximum current, and the insulation being such as to allow a certain maximum potential difference between the mains, you increase the cost of putting up the wires by 50 per cent., but you increase the output by 73 per cent. over two wires and a single-phase distribution of motive power. So that, in fact, so far from increasing the cost,—so far from there being less output

obtainable from a given weight of material with the three-wire system, there is actually a greater output. Professor Ayrton.

Then the author says: "The average pressure acting in "the closed coils will be given by the expression $K\,C\,F\,a\,10^{-8}$, "where K equals a constant, C the number of turns counted "all round the periphery of armature." I do not understand what exactly he means by C, and I should like further explanation. In the Drehstrom motors the wires on the armature are short-circuited; they are therefore, in a sense, in parallel, and I do not see how the average pressure acting in a closed coil can depend on the number of turns of wire counted all round the armature. The pressure in the coil would be the same, as far as I understand it, however many wires you had round the armature. It is not like an ordinary direct-current motor or dynamo where the coils are in series. Here they are all in parallel. A little lower down the author says: "Now the torque will vary with the ampere-turns, the frequency "of the current, and the number of lines of force in the armature; "or, the torque $= \frac{C\,i_a\,F\,a}{K_1}$, where $i_a =$ the armature current, and "$K =$ a constant, and the other symbols have the same signification "as above." I do not quite understand that again; I do not see how the armature current and the frequency can both come in; I do not see, in fact, how the torque can depend upon the frequency as well as upon the armature current. Of course the frequency—that is, the frequency due to slip—the difference in the number of rotations per second of the rotating field and the armature, determines the electro-motive force set up in the short-circuited wires on the armature, and so determines the value of the current in the armature coils. When you have got the value of the current in the armature coils, I do not see why in the formula you want "a," which is defined above as "the number "of alternations per second of the armature induced currents."

There is an interesting point on page 305, where Mr. Snell is making experiments with the Gramme machine, used as a three-phase dynamo. He says: "It will also be seen that the direct "pressure is higher than that of the alternating in the proportion

Professor Ayrton.

"of 158 to 94 on open circuit." What interests one, when one studies these two numbers, is to see how nearly they would be given by a sine law of variation of electro-motive force with position of the armature relatively to the field magnet.

I have therefore made the following calculation to determine the root of the mean square of the electro-motive force, or the R.M.S.E.M.F., as it may be shortly called, produced by the coils in any segment of a Gramme armature on open circuit when the field magnets are separately excited.

Let B B (Figs. 1, 2) be the line passing through the main brushes of the machine, N, S, the field magnets; and let a segment of the armature contained between the planes O P, O Q, making an angle ϕ with one another, have its ends connected with rings, R, R, which are continuously rubbed by fixed brushes. Let n be the number of windings per radian on the armature, e the maximum E.M.F. per convolution: then, if the sine law be true, the E.M.F. set up in any one convolution in a plane making an angle θ with the plane passing through the brushes will be

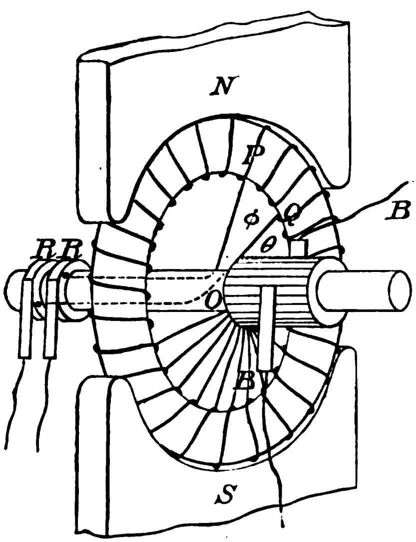


FIG. 1.

$$e \sin \theta;$$

therefore the E.M.F. in the segment contained between the planes O P, O Q, will be

$$\int_{\theta}^{(\theta + \phi)} n\, e \sin \theta\, d\, \theta,$$

or, $$n\, e \{\cos \theta - \cos (\theta + \phi)\}.$$

Consequently the mean square of the electro-motive, or the M.S.E.M.F. as it may be called, will be

$$\frac{n^2 e^2}{2\, \pi} \int_0^{2\pi} \{\cos \theta - \cos (\theta + \phi)\}^2 d\, \theta;$$

or, $$n^2 e^2 (1 - \cos \phi).$$

Hence, when the segment of the armature is contained between planes making angles respectively of $\frac{\pi}{3}, \frac{\pi}{2}, \frac{2\pi}{3}, \pi$; the M.S.E.M.F. will have the values respectively, Professor Ayrton.

$$\frac{n^2 e^2}{2}, n^2 e^2, \frac{3 n^2 e^2}{2}, 2 n^2 e^2;$$

and therefore, if the external circuit be open, a hot-wire, or

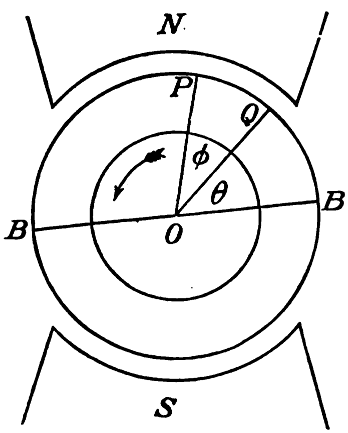


FIG. 2.

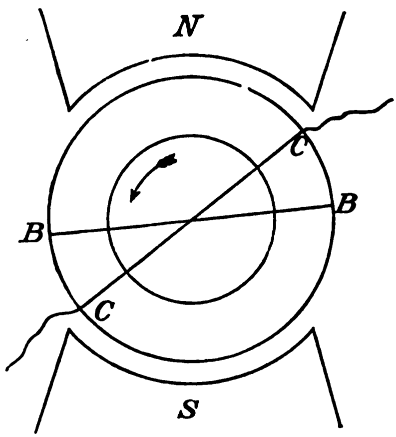


FIG. 3.

electrostatic, voltmeter attached to the brushes rubbed by the rotating rings will measure

$$\frac{n e}{\sqrt{2}}, n e, \sqrt{\frac{3}{2}}\, n e, \text{ or } \sqrt{2}\, n e,$$

according as these rings are connected with a rotating segment of the armature contained between planes making an angle of $\frac{\pi}{3}, \frac{\pi}{2}, \frac{2\pi}{3}$, or π with one another.

If, on the other hand, the voltmeter be attached to the main brushes of the armature, it will measure a steady E.M.F. whose value will be

$$\int_0^\pi n e \sin\theta \, d\theta,$$

or,

$$2 n e.$$

Consequently, the E.M.F. measured by the voltmeter attached to the main brushes of the machine will bear to the E.M.F. measured by the voltmeter attached to the rings the ratios,

$$2\sqrt{2}, 2, 2\sqrt{\frac{2}{3}}, \sqrt{2},$$

Professor Ayrton.

according as the rings are attached to points on the armature at an angular distance of $\frac{\pi}{3}$, $\frac{\pi}{2}$, $\frac{2\pi}{3}$, or π.

Now, in Mr. Snell's experiment, $\frac{2\pi}{3}$ was the angular distance between the points on a segment of the armature attached to adjacent rings; therefore, as the R.M.S. of the alternating E.M.F. was 94 volts, the steady E.M.F. between the main brushes of the machine would, on the assumption that the sine law were true, equal $2\sqrt{\frac{2}{3}} \times 94$, or 154 volts. And as Mr. Snell found practically the same value, 158 volts, it follows that when the machine is running on open circuit the sine law of variation of E.M.F. of a convolution with its position relatively to the line B B passing through the main brushes, holds.

When, however, a current of 20 amperes was taken from each of the three segments of the armature, the ratio, he found, was increased. This is consistent with the experiments made some time ago by students at the Central Institution on our Gramme machine, which has points on its armature joined to six different rings so that two alternating currents differing by 90° in phase, or three alternating currents differing by 120° in phase, can be obtained in addition to the ordinary direct current.

The experiments to which I refer consisted in comparing the P.D. between the main brushes with the R.M.S. of the P.D. between two rings attached to two points on the armature at the ends of a diameter. ϕ in this case was therefore π, and consequently the ratio of the P.Ds., as already proved, should be $\sqrt{2}$ when the machine was running on open circuit. This ratio was found to hold approximately when experiments were made on open circuit with the engine driving the dynamo at four different speeds. But when an alternating current was taken from the rings the ratio increased as the current increased, this increase in the ratio for a given current being the greater the larger the self-induction placed on the external circuit. For example, when there was considerable self-induction in the external circuit, the ratio, which for open circuit was $\sqrt{2}$, or 1·434, increased up

to 2·56 when the R.M.S. current was only 11 amperes. When, on the contrary, the external circuit contained capacity instead of self-induction, the ratio of the P.D. at the main brushes to the P.D. at the rings diminished instead of increasing as the current was raised. Professor Ayrton.

The explanation of these changes is not difficult to see. The main brushes were always adjusted so that the P.D. between them had its maximum value, which was also the maximum value of the alternating P.D. set up at the ends of the segment of the rotating armature, seeing that the rings attached to points at the opposite ends of a diameter were used to produce the alternating current in these special experiments.

But owing to the self-induction of the armature the alternating current will attain its maximum a little later than the E.M.F. attains its maximum, so that, if C C (Fig. 3) represent the two points of the armature attached to the rings, it will be when the line C C is in some such position as that shown in Fig. 3 that the alternating current which is taken off from the two points C C by means of the rings will have its maximum value.

The currents flowing round the armature produce magnetic poles at the points C C which weaken the magnetic field, and the more the line C C is shifted round in the direction of rotation of the armature before the alternating current attains its maximum value, the greater will be the weakening of the field on the whole. Consequently, the greater the self-induction on the circuit, the greater will be the weakening of the field on the whole for a given value of R.M.S. current, and therefore the smaller will be the R.M.S.P.D. The maximum P.D. will also be diminished somewhat as the current is increased, since the main brushes of the dynamo have some positive lead, but the R.M.S.P.D. will be diminished much more rapidly than the maximum P.D.; and therefore the ratio of the P.D. at the main brushes to the P.D. at the rings will increase both as the current increases and as the self-induction of the circuit increases.

If the external circuit, on the contrary, has capacity given to it, the line C C will be twisted back against the direction of

Professor Ayrton.

rotation, and so the average weakening of the field will be diminished; indeed, with the large capacity of 60 microfarads used in these experiments at the Central Institution, the capacity more than counterbalanced the self-induction of the armature, so that the line C C, instead of being in front of the line B B, was actually behind. Under these circumstances increase of current will strengthen the field, so that the ratio of the P.D. at the main brushes to the P.D. at the rings diminished as the current increased.

Returning now to Mr. Snell's paper: If he has an opportunity of repeating his experiments, it would be most valuable if, instead of merely measuring current and P.D., and calculating the *apparent* power, he uses wattmeters, and ascertains the *true* power dealt with in the different tests. For in alternating-current problems, unless you can get the true watts—not merely the apparent watts—you do not obtain nearly as much information from the experiment as would otherwise be available. At the same time, the paper contains a number of practical results, and Mr. Snell has endeavoured to put the well-known theoretical considerations about Drehstrom motors before you in a simple fashion. I think, therefore, we have to thank him for his contribution.

Mr. Snell.

Mr. SNELL (in reply): Mr. President,—The subject is bristling with undetermined points of theory and practice, and I am afraid I cannot do justice in an immediate reply to the various speakers who have done me the honour to criticise the paper. But there are one or two points that I should like to refer to. Mr. Sparks has put before the Institution this evening a very interesting motor, to which I referred in my paper. I think, if Mr. Sparks had read the amended proof, he would have seen that I had done justice to the machine as it stands. And I cannot agree with him that the credit of the invention is entirely due to Mr. C. E. L. Brown; for, after all, it is, to my thinking, nothing but a development of Professor Elihu Thompson's motor.

With reference to Mr. Brown's single-phase non-synchronous motor, that undoubtedly has advanced the subject very materially, but I still believe in the truth of the concluding paragraph of my

paper: "This is perhaps the best type of single-phase motor up "to date, but it is undoubtedly susceptible of considerable improve-"ment, and the problem of alternate-current motors suitable for "running on ordinary supply circuits cannot yet be said to be "satisfactorily solved." I think the majority of speakers this evening supported me on this point. We are all of us indebted to Mr. C. E. L. Brown for his labours in connection with dynamo construction, and one of his most important contributions is the embedding of the winding in tunnels in the iron. This enables the magnetic resistance of the gap to be reduced to small limits, and consequently increases the weight output efficiency, assuming, of course, sparking can be controlled. I referred to the importance of the periodicity, but not with sufficient emphasis to please Mr. Mordey. Yet I stated that the crux of the problem lay in the frequency; and undoubtedly until we can make a single-phase motor which will work on a circuit of 100 cycles per second, we have not commercially solved the problem. At the time when I wrote the paper I was thinking about motors to be used in coal pits, where the desideratum is not so much a motor which will work at a high efficiency as one which will work without sparking. The little motor shown on the table realises these fundamental conditions. If the efficiency of that motor is only 60 per cent., it will do for a great many purposes in mining. Mr. Snell.

If a larger motor can be built with an efficiency of from 70 to 80 per cent., we may be said to have materially advanced the question of the application of power to mining work. Lighting in a coal pit is a secondary matter altogether, for the lighting is subsidiary to the transmission of the power for driving machines. With reference to the definition of the terms "field" and "arma-"ture:" In the paper, I copied Continental practice; that is, I called the part of the machine which has currents induced in it the "armature," and the "field" that which, having the Drehstrom current supplied to it, gives rise to the magnetic field. That being the case, I call the stationary part on Mr. Brown's motor the "field magnets," and the part that revolves, the "armature," because it has currents induced in it. The converse is also easily seen. We might have a stationary armature on the outside and

Mr. Snell. a revolving field in the centre. But, undoubtedly from purely theoretical considerations, Mr. Swinburne's suggestions, as amended by Professor Ayrton, are well worth considering, and perhaps may obtain in the future; but in the paper, and for the present, as I have clearly given the definitions of my terms, I think there is no need for confusion. I am sorry that the discussion has not elicited more practical data from America and the Continent; for although it is the custom just now for English engineers to regard multiphase working as a needless complication, yet, even if regarded as a necessary step to a simpler type of motor, it is worthy of the best attention. The Frankfort-Lauffen plant was, it is true, only a *tour de force*, but it did good service in demonstrating certain points of theory and practice. We are undoubtedly on the eve of further developments in the distribution of electric power.

Mr. Siemens. The CHAIRMAN: I now move that the meeting accord it best thanks to Mr. Snell for the very interesting paper, which has given rise to an equally interesting discussion. I cannot help emphasising one point which has been brought out, viz., that the English engineer should not be too much blamed for holding back in this question of rotary currents. After all, we are practical people, who apply science to everyday life. Our objects are not merely scientific investigations, but we have to consider the eternal question of £ s. d. I may just mention a personal experience. Our firm in Berlin is very much interested in rotary motors, and has worked a good deal in that direction. In several instances when we over here had to work out problems for transmitting power to a distance, we have given the exact data to our Berlin house, and they have worked them out according to the rotary system; and up to the present time we have not had an instance in which we could not carry out the problem just as well by direct currents or by the ordinary alternating currents as they could by rotary currents. This evening the probability has been stated that the rotary-current motors are only an intermediate step, and that we may expect to have a thoroughly practical alternating-current motor which can be used on the ordinary distributing mains: then we shall be able to distribute power

by alternate currents in the same way as we do now by continuous currents. However this may turn out, I think the thanks of the Institution are due to Mr. Snell for bringing the subject forward. Mr. Siemens.

The motion was unanimously carried.

The CHAIRMAN: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Associates:

M. A. Abrahamson.	Joseph Gritton.
George Bloomfield Garvey.	Edward H. Tyler.

Students:

Arthur Francis Berry.	John Stanley Plumtree.
Arthur Ernest Cullen.	Frederick Simpkin.

The meeting then adjourned.

ORIGINAL COMMUNICATIONS.

ON A GRAPHIC METHOD OF STUDYING THE BEHAVIOUR OF THE EXPOSED ENDS OF BROKEN TELEGRAPH CABLES, AND OF MEANS OF ELIMINATING THE EFFECT OF EARTH CURRENTS, AND OF POLARISATION AT THE FAULT.*

By G. K. WINTER, M. Inst. C.E., Member,
and
G. B. WINTER, Associate.

It is, we think, necessary, after the valuable papers that have been read by Mr. (now Sir Henry) Mance† and by Mr. Kennelly‡ on the subject, to offer an apology for again referring to a question that has been already so ably discussed by the Institution of Electrical Engineers. There remains, however, the fact that Sir Henry Mance's and Mr. Kennelly's methods are founded on two suppositions which are at variance with one another. We gather from Sir Henry Mance's paper that he takes it for granted that the E.M.F. of polarisation at, and the resistance of the fault remain the same during the two tests with different proportional resistances in the bridge, and consequently with different currents passing through the fault. This is, of course, supposing that the resistance of the line and fault is measured in the ordinary way of resistance measuring; and his object in using different proportional resistances was to eliminate the false apparent resistance due to polarisation and earth currents from the results obtained.

Mr. Kennelly, in his method, attempts to eliminate the effects of polarisation on the apparent resistance of the fault by the immediate false zero method, and in doing so has, by varying the value of the testing current, been led to the

* To prevent unnecessary repetition, this expression is held to include all E.M.F. at the fault.

† *Journal of the Institution*, vol. xiii., p. 328.

‡ *Journal of the Institution*, vol. xvi., pp. 219, 581.

conclusion that the resistance of the fault is not constant, but varies inversely as the square root of the current passing through it.

Many years ago—about 1873 or 1874—the subject was taken up by one of us, and the conclusion arrived at, after a long series of experiments, was that the resistance of the fault remained unchanged, whatever current was passing through it, and that the apparent change in resistance, with different electro-motive forces in the testing battery, was entirely due to the effects of polarisation; and this was the principal reason for not publishing the empirical formula which was referred to by Professor Jamieson in the *Electrician* of 6th May, 1887.

After seeing Mr. Kennelly's paper, which was read before the Institution in 1887, we took up the subject together, and we hope that, by investigating the subject graphically as well as experimentally, we have been able to throw some new light on a somewhat abstruse question.

Before proceeding to a consideration of the method to be brought forward in this paper, it will simplify matters if we explain, as clearly as we can, the principle on which it is founded, and for this purpose we have prepared Fig. 1.

In this figure resistances are represented along the horizontal line $a\ f$, and differences of potential by vertical lines drawn from it, such as O E, O E′, O E″, $f\ e$, and $f'\ e'$.

We will suppose the point O to represent the fork of a Wheatstone's bridge, and from this point as zero the resistances will be reckoned on the right-hand side towards f, and on the left-hand side towards a.

On the right-hand side, O p represents the resistance of one proportional coil, and $p\ f$ the resistance of the line and fault, $p\ l$ representing that of the line, and $l\ f$ that of the fault. At the fault let there be a difference of potential between the conductor and the earth, represented by the line $f\ e$.

On the left-hand side, O P represents the resistance of the other proportional coil (we will suppose that the proportional coils are equal), and P a, P b, and P c represent the resistances in the balancing coil in the three tests, when the galvanometer, G, connected to the two points P and p is at zero.

At the point O erect a perpendicular, O E″, and let the heights of the points E, E′, and E″ above the point O represent the differences of potential between the fork of the bridge and the earth, in three different tests with different battery powers, or with different resistances in the battery circuit.

From the points E, E′, and E″ draw the straight lines E *e*, E′*e*, and E″*e* to the point *e*. These lines will represent the falls in potential along the proportional coil O *p*, and the line and fault *p f*, in the three tests.

At the point *p* erect a perpendicular, cutting these falls in potential at the points v, v′, and v″. These three points will represent the potential at the point *p* in the three tests. At the point P erect another perpendicular, P V″, and on it take the points V, V′, and V″, such that P V, P V′, and P V″ are equal to *p* v, *p* v′, and *p* v″ respectively. These three points, V, V′, and V″, will represent the potential at the point P in the three tests when balance on the galvanometer is produced. Join the points E v, E′v′, and E″v″, and produce these lines respectively to *a*, *b*, and *c*. These lines represent the falls of potential along the proportional coil O P, and the balancing resistances P *a*, P *b*, and P *c*, and it is evident that they will intersect at the point *e′*. If from *e′* we drop a perpendicular, *f′e′*, on to the line P *a*, this line *f′e′* will be equal to the line, *f e*, representing the difference of potential between the fault and the earth. P *f′* will be equal to *p f*, representing the resistance of the line and fault, and *f′a*, *f′ b*, and *f′c* will represent the extra resistances added in the balancing coil in order to obtain balance in consequence of the difference of potential, *f e*, between the fault and earth.

In applying the principle we have described to localising faults in cables, it is evident that the E.M.F. of polarisation at, and the resistance of the fault must be supposed to remain constant, otherwise the lines representing the falls in potential would not intersect in one point. We hope to show that, although the intersection is not absolutely in one point when the testing current varies largely, still it is so within fairly wide limits; and that the variation is, in any case, small.

In the first instance, the currents passing through the fault

were varied by varying the E.M.F. of the battery; but this method, involving as it did the measurement of the battery resistances, and the electro-motive forces of the several batteries employed, was given up in favour of the much simpler method of altering the resistance of the battery branch. This method was first suggested by Professor Ayrton, in the discussion on Mance's paper in 1884, but we are not aware of its having hitherto been practically worked out. Kennelly also used the method of introducing a varying resistance into the battery branch, but for a different purpose. His object was to vary, by definite amounts, the current passing through the fault; our object, on the other hand, is to make use of the resistance added to the battery branch as a factor in determining the resistance up to and through the fault.

Fig. 2 shows the plotting of one set of results obtained by adding successively equal resistances into the battery branch. In this figure, as in Fig. 1, resistances are reckoned along the horizontal line, and electro-motive forces are represented by vertical lines. The battery used was the same throughout, and consisted of 12 Daniell's cells.

Say we begin at the point marked O on the horizontal line; then going to the right we have first a measured part, equivalent to twice the battery resistance, or 188 ohms; next we have 100 ohms, the resistance of one of the proportional coils; we have, lastly, a series of measured resistances reaching from the end of the proportional coil to from 577 ohms to 687 ohms. The inserted resistance in this case was 500 ohms, so that the apparent resistance of the fault varied from 77 to 187 ohms.

From the point O a vertical line, O E, is drawn representing the E.M.F. of the testing battery; and going to the left from this point are a series of points, at equal distances apart, from each of which points vertical lines representing the E.M.F. of the testing battery are drawn; these distances along the horizontal line represent, in each case, twice the resistance added to the battery branch. (These resistances, as well as the battery resistance, are doubled for a reason which is explained in Appendix I.)

From the summits of the lines representing the E.M.F. of the testing battery, with different resistances added to the battery branch, lines are drawn to the corresponding measured resistances, and it will be seen that these lines cut each other very nearly at the same point, *e*. Dropping a perpendicular line from *e* on to the horizontal line, we get the point *f*, and the distance between this point and the end of the proportional coil gives us the resistance up to and through the fault. The distances between *f* and the points from 577 to 687 represent the extra resistances in the different tests due to the E.M.F. at the fault.

In order that the lines representing the falls in potential should accurately intersect in one point, it is evident, from the construction of the figure, that, supposing equal resistances are added successively to the battery branch, the successive increments in the corresponding measured resistances should be equal also. To show how far this condition is fulfilled, we give the following example:—

Table I.

Length of wire exposed, ½ inch. Line resistance, 500 ohms. Earth plate galvanised iron. Electrolyte salt water.

Resistance added to Battery Branch.	Measured Resistance of Line and Fault.	Difference between each Two Consecutive Readings.
0	577	...
100	589	12
200	601	12
300	613	12
400	624	11
500	635	11
600	646	11
700	657	11
800	667	10
900	677	10
1,000	687	10

The gradually diminishing increments in the measured resistance, as successive additions are made to the battery branch, would appear to show either that the resistance of the fault, or the E.M.F. of polarisation, or both together, decrease slightly with the decrease of current passing through the fault. This appears to be probable, as the generation of hydrogen certainly decreases, and as a natural consequence the surface of wire exposed to the electrolyte increases as the current decreases. The polarisation certainly increases with the current while the current is small, but it appears to tend towards a maximum with a value of current which probably varies with the area of wire exposed, and consequently with the resistance of the fault.

If, however, we calculate the resistance of the fault by the formulæ to be given presently, taking as one of our tests the measured resistance without any additional resistance in the battery circuit, and as our second test each one in succession of the series of measured resistances, with resistances added to the battery circuit, as given in Table I., we find that the resistance of the fault gradually rises as the current diminishes; and, in this, the method agrees with Mr. Kennelly's results. It must be pointed out, however, that a decrease in the E.M.F. of polarisation combined with a decrease in the resistance of the fault as the current decreases is not incompatible with an apparent increase in the resistance of the fault as calculated in this way. This and some other points are still under investigation, and it is hoped that further results will be given either in an appendix to this, or in a subsequent paper.

The following table gives the resistance of the line and fault calculated from the results given in Table I.:—

Table II.

Resistance added to Battery Circuit.	Calculated Resistance of Line and Fault. By Formula 2.	Calculated Resistance of Line and Fault. By Formula 4.	Resistance of Fault, Mean of the Two.	
0	...	...	...	The approximate formula gives results somewhat lower than those given in the table.
100	528	528	28	
200	528	528	28	
300	528	528	28	
400	528	529	28·5	
500	529	529	29	
600	529	529	29	
700	530	530	30	
800	531	530	30·5	
900	531	531	31	
1,000	531	531	31	

The variation from equal increments in the measurements themselves as equal resistances are added to the battery branch, is so small that, at first, we were inclined to look upon it as due, either to imperfection in the resistance coils, or to errors in observation; but further investigation showed that the variation is real, though small enough to be neglected for our present purposes; so that, with currents not very different from each other, the lines representing the falls in potential will practically intersect in one point, and, this being granted, it is evident that only two tests are required to determine the point of intersection, either graphically or mathematically.

An example of the graphic method is shown in Fig. 3. Having found the point of intersection, the resistance up to and through the fault is the resistance represented by that part of the horizontal line between the end of the proportional coil, and the point vertically below the point of intersection.

Assuming, as sufficiently correct for our present purpose, that the point of intersection is constant when the currents used in our tests are not very different from each other, or, in other words, that the variations of the E.M.F. of polarisation and of the resistance of the fault are small enough, under these circumstances, to be neglected, the following formula will enable us to determine the resistance up to and through the fault. (See Appendix II.)

Let $A =$ the measured resistance without resistance added to the battery branch;

$B =$ the measured resistance with a resistance added to the battery branch;

$a =$ the resistance of the proportional coil plus twice the battery resistance;

$b =$ the resistance of the proportional coil plus twice the battery resistance, plus twice the added resistance.

Then the resistance up to and through the fault is,

$$x = \frac{A\,b - B\,a}{B + b - A - a} \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

From this formula, by an easy substitution, Mance's formula, as given in his paper in 1884, is at once obtained. (See Appendix III.)

This formula is sufficiently simple, but there is another still simpler.

Assuming, as we have seen we may do, that, within the limits of our testing currents, the ratio between the resistance added to the battery branch and the extra resistance observed is constant, then, if r be the resistance added to the battery branch, the ratio $\frac{B - A}{2\,r}$ will be constant.

Let fraction $$\frac{B - A}{2\,r} = K.$$

Then we will show in Appendix IV. that

$$x = \frac{A - K\,a}{1 + K} \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

or, approximately, $$x = A - K\,(A + a) \quad \ldots \quad \ldots \quad (5)$$

We have hitherto taken the E.M.F. at the fault as opposing

the testing current; should it, however, be in the same direction as that of the testing battery, then the measured resistances will be smaller than the true resistance, and the formula will be

$$x = \frac{A + K a}{1 - K} \quad \dots \quad \dots \quad \dots \quad (6)$$

or, approximately,

$$x = A + K(A + a) \text{ (see Appendix IV.)} \quad (7)$$

K in this case will be $\frac{A - B}{2r}$, as A will be larger than B. In fact, we may take the relative values of A and B as indicating the sign of the E.M.F. at the fault.

If B is the greater, then the E.M.F. at the fault opposes the testing current.

If A is the greater, the E.M.F. at the fault aids the testing current.

So far we have only dealt with the resistance up to and through the fault, but have made no attempt to separate the line resistance from that of the fault. This is the most difficult part of the problem, but we do not consider it insoluble. It is probable that the small variation from equality in the increments of the measured resistances will enable us at all events to estimate the resistance of the fault, and we shall be probably further aided in this by finding the current which raises the E.M.F. of polarisation to its maximum value. It seems to us, however, that by putting an artificial fault at the end of the balancing resistance, we not only eliminate directly the false resistance due to polarisation, but by altering the size of the exposed wire at the artificial fault until the measured resistance remains the same, whether the testing current used be small or large, we may arrive, without calculation, at the result aimed at. It is most likely that this, or some similar method, was used by the pioneers in submarine telegraphy, and, simple as it is, it seems never to have been even suggested in the papers or discussions on the subject. We hope to be able to return to these two points later on.

It may perhaps be of some interest to show the application of the graphic method described in this paper to some of the results

Mr. Latimer Clark gave in introducing Sir H. Mance's paper in 1884. As, however, he gave neither the method of measurement, nor the E.M.F. or resistance of his testing batteries, we are rather at a loss to plot the results correctly. We have, however, after making a few trials, arrived at what we hope is a fairly satisfactory solution. Supposing the results given by Mr. Clark to be in ohms,* and the battery used to have consisted of Daniell's cells, the method adopted appears to have been the deflection method, as if either the bridge or the differential galvanometer method were used the internal resistance of the battery must have been only about 2·5 to 3 ohms per cell, whereas if the deflection method were used this resistance would have been 5 or 6 ohms. Lastly, judging from the value of the E.M.F. of polarisation, we believe the earth plate must have been of copper.

Figures 4, 5, 6, and 7 represent four of Mr. Clark's results. These figures explain themselves, but we may tabulate the results as follows:—

Length of Wire exposed.	Resistance of Fault as found by this Method.
$\frac{1}{8}$ in.	17 ohms.
$\frac{1}{4}$ "	10 "
$\frac{3}{8}$ "	8 "
$\frac{1}{2}$ "	6 "

It is perhaps necessary to point out that all measurements referred to in this paper were taken with zinc to line.

It is obvious that the use of the method is not confined to cable-testing, but that it affords a new means of measuring the electro-motive force and the resistance of batteries, the polarisation and resistance of electrolytes, &c. A paper on this subject, together with some interesting results already obtained by the method, by Messrs. G. K. Winter and Rungacharryar, M.A., will, it is hoped, be ready before the former returns to India.

* Note.—It must be remembered that in the early days Varley's unit of resistance was usually employed in England, and Siemens's on the Continent.

APPENDIX I.

Explanation of the reason why the resistance in the battery branch has to be doubled in plotting the results:—

Let E = Electro-motive force of the testing battery;

β = the resistance of the testing battery;

P = the resistance of one of the proportional coils, which we will suppose to be equal to each other;

A = the resistance observed;

C = the whole current given by the battery;

C_1 = the current passing through the fault.

Then when balance is obtained the whole current given by the battery will be

$$C = \frac{E}{\beta + \frac{1}{2}(P+A)}.$$

The current passing through the fault will be one-half of this, or

$$C_1 = \frac{1}{2}\,\frac{E}{\beta + \frac{1}{2}(P+A)},$$

or

$$C_1 = \frac{E}{2\beta + P + A} \qquad \ldots \qquad \ldots \qquad (1)$$

so that in plotting the current through one branch of a Wheatstone's bridge, when balance is obtained, although E, P, and A are plotted as if there was no division of the current, β, the battery resistance, must be doubled. This applies also to any resistance added to the battery branch.

APPENDIX II.

Let E, in Fig. 8, represent the electro-motive force of the testing battery;

e the electro-motive force at the fault, which we will suppose opposes that of the testing battery;

A the measured resistance when no resistance is added to the battery branch;

B the measured resistance when a resistance is added to the battery branch;

a the resistance of the proportional coil plus twice the battery resistance;

b the resistance of the proportional coil plus twice the battery resistance and twice the added resistance;

x the resistance of the line up to and through the fault.

Then, by similar triangles,

$$\frac{e}{E} = \frac{A - x}{A + a};$$

also,

$$\frac{e}{E} = \frac{B - x}{B + b},$$

$$\frac{B}{B + b} - \frac{x}{B + b} = \frac{A}{A + a} - \frac{x}{A + a},$$

$$\frac{x}{A + a} - \frac{x}{B + b} = \frac{A}{A + a} - \frac{B}{B + b},$$

$$x\left(\frac{1}{A + a} - \frac{1}{B + b}\right) = \frac{A}{A + a} - \frac{B}{B + b},$$

$$x\{(B + b) - (A + a)\} = A(B + b) - B(A + a),$$

$$x = \frac{A(B + b) - B(A + a)}{(B + b) - (A + a)},$$

$$x = \frac{Ab - Ba}{B + b - A - a} \quad \ldots \quad (2)$$

APPENDIX III.

MANCE'S TEST.

In this test, instead of adding resistance to the battery branch, we alter the proportional resistance. In the above we have taken a and b to be equal to the proportional resistance plus twice the resistance in the battery branch when the resistances measured were A and B respectively.

If P_1 and P_2 are the proportional resistances respectively when the measured resistances are A and B, and β = the battery resistance, then

$$a = P_1 + 2\beta,$$

and

$$b = P_2 + 2\beta.$$

Substituting these in equation (2), we get,

$$x = \frac{A(P_2 + 2\beta) - B(P_1 + 2\beta)}{B + P_2 + 2\beta - A - P_1 - 2\beta},$$

$$x = \frac{A(2\beta + P_2) - B(2\beta + P_1)}{P_2 + B - P_1 - A} \quad \ldots \quad (3)$$

which is Mance's formula.

APPENDIX IV.

We will first suppose that the electro-motive force at the fault opposes that of the testing battery.

Adopting the same notation as before, and noting that r = the resistance added to the battery branch, and that a = the proportional coil plus twice the resistance of the battery, we have, by similar triangles,

$$\frac{E}{e} = \frac{2r + a + B}{B - x},$$

and
$$\frac{E}{e} = \frac{A + a}{A - x},$$

$$\frac{2r + a + B}{B - x} = \frac{A + a}{A - x},$$

$$(A - x)(2r + B + a) = (B - x)(A + a),$$

$$A\,2r + A\,a - 2rx - xB = B\,a - x\,A,$$

$$B\,a - A\,a + B\,x - A\,x = 2r\,A - 2r\,x,$$

$$(B - A)(a + x = 2r(A - x),$$

$$\frac{B - A}{2r} = \frac{A - x}{a + x}.$$

Let
$$\frac{B - A}{2r} = K:$$

then
$$K = \frac{A - x}{a + x},$$

and
$$x = \frac{A - K\,a}{1 + K} \quad \ldots \quad \ldots \quad (4)$$

As K is a small fraction, we may write this approximately,

$$x = (A - K\,a)(I - K);$$

and as $K^2_j\,a$ will be very small, we have,

$$x = A - K(A + a) \quad \ldots \quad (5)$$

Let us now suppose that the electro-motive force at the fault is in the same direction as the testing current. The plotting of the test will now be as shown in Fig. 9. A will be greater than B, and the measured resistances will both be less than x, the actual resistance up to and through the fault.

$$\frac{E}{e} = \frac{a + A}{x - A},$$

$$\frac{E}{e} = \frac{2r + a + B}{x - B},$$

$$\frac{a + A}{x - A} = \frac{2r + a + B}{x - B},$$

$$A\,x - a\,B = 2\,r\,x - 2\,r\,A - A\,a + B\,x,$$

$$A\,x - a\,B + A\,a - B\,x = 2\,r\,(x - A),$$

$$(A - B)\,(x + a) = 2\,r\,(x - A),$$

$$\frac{A - B}{2\,r} = \frac{x - A}{x + A}.$$

$$\text{Let } \frac{A - B}{2\,r} = K:$$

then

$$K = \frac{x - A}{x + a},$$

$$x = \frac{A + K\,a}{1 - K} \qquad \ldots \qquad \ldots \qquad (6)$$

As K is a small fraction, we may write this approximately thus,

$$x = (A + K\,a)\,(1 + K);$$

and as $K^2\,a$ will be very small,

$$x = A + K\,(A + a) \qquad \ldots \qquad (7)$$

SOME ADVANTAGES OF CONNECTING THE COILS OF ELECTRO-MAGNETS IN MULTIPLE ARC.

By F. Higgins, Member.

If two exactly similar electro-magnets of, say, 10 ohms resistance each and 1,000 turns of wire be employed, and one be connected in series in the usual way, and the other in multiple arc, the resistance of the latter will by so doing be reduced to 2·5 ohms.

With a current of 0·5 ampere in the circuit the second electro-magnet will be magnetised by 250 ampere-turns, and the series coils by 500 ampere-turns; but the potential difference involved in producing the effect in the magnet connected in multiple arc is not a half, but only one-quarter, of that required for the series magnet. Thus:

$$\frac{5 \text{ volts}}{10 \text{ ohms}} = 0{\cdot}5 \text{ ampere} \times 1{,}000 \text{ turns} = 500 \text{ ampere turns};$$

$$\frac{1{\cdot}25 \text{ volts}}{2{\cdot}5 \text{ ohms}} = 0{\cdot}5 \text{ ampere divided between two coils of 500 turns each, or } 0{\cdot}25 \times 1{,}000 = 250 \text{ ampere-turns.}$$

That is, 2·5 watts in the first case;
0·625 „ „ second „

A further advantage where speed is a consideration is that the induced discharge on closing the circuit being of much lower intensity, is more readily overpowered by the battery, and the magnetisation more promptly established. The discharge on opening circuit being also proportionately lower in potential, is more confined to the bobbin.

Thus it will be seen that by this method of connecting electro-magnets, more effect for a given current can be obtained under certain conditions, or two magnets may be made to do the work of one, at half the total expenditure of current required by a single similar magnet, and with greater promptitude in both charging and discharging.

Diagram p

See Journa

Electromoti

Resistance

6 cells and

$\frac{1}{4}$ inch exp

Resistance

ABSTRACTS.

F. UPPENBORN—THE CENTRAL STATIONS OF SCHUCKERT & CO.: II.—HANOVER.

(*Elektrotechnische Zeitschrift*, *No.* 9, 1893, *p.* 105, *No.* 13, *p.* 173.)

This station was opened for electric supply on March 3rd, 1891, and carries into further development the principle used at Barmen of using cells constantly in parallel with the dynamos, the volts on the mains being kept constant by means of the discharge switches; and at Hanover the cells are approximately of the same output as the dynamos. The station is a very fair sized one, being designed for 44,000 lamps—say 1,300 kilowatts. The boilers are of ordinary type; the engines, of which there are three (two of 400 and one of 600 H.P.), are triple-expansion condensing engines of the vertical type, the cylinders being arranged along the length of the crank-shaft, and working three cranks at 120° to one another, and the speed being 120 revolutions per minute. The armature of the direct coupled dynamo serves as a fly-wheel, and the whole arrangement is said to produce a very steadily running combination. To receive the dynamos, since the shaft of the engine is placed just on the bed-plate, the floor of the engine room is built on two different levels, and the armature runs in its lower part in a hole in the floor. The dynamos are interesting: they have disc armatures 10 feet in diameter, and are multipolar, having 16 poles; the current is taken off by eight sets of brushes on a commutator 6 feet 6 inches in diameter, and having 840 sections. The cells are arranged across the mains on the three-wire system, the middle wire coupled to the middle cell, and the two outer to the discharge switches. There is a discharge switch for each feeder, each capable of independent regulation. These discharge switches are of massive and ingenious construction, and are so arranged as to prevent short-circuiting a cell when passing over from one cell to the next, and at the same time to keep a heavy pressure on the sliders, and are automatically regulated by small motors, but are also capable of hand regulation. It has been found, however, that the necessary regulation is so minute that it suffices to adjust the switches occasionally by hand. The cable system has no points of particular interest. The network is of double lead-covered cable, which is further protected by a covering of iron tape, the feeders being laid in channels of U iron with flat cover, as are also the supply cables at street crossings.

A point of general interest in this station is the extensive system of statistics kept here. We give a few figures from the results of the first year's running—April 1, 1891, to March 31, 1892. The three boilers ran altogether 4,630 hours, and used 890 tons of coal, of which 23·5 per cent. was for getting up steam. The coal used was Westphalian anthracite, burning with 8-10 per cent. residue. The efficiency of the steam dynamo averages 79·4 per cent. Two steam dynamos ran altogether 2,433 hours, and gave in that time 437,463 kilowatt-hours. These results give a consumption of 3·02 lbs. of coal per horse-power-hour (including getting up

steam). The accumulators supplied 43·6 per cent. of the energy to the mains, and had an average efficiency (in watt-hours) for the year of 78 4 per cent., from half-hourly observations. The cells are always charged until they gas freely, and every week are overcharged for several hours.

355,364 kilowatt-hours were delivered to consumers, being 178 watt-hours per lb. of coal. The percentage of the maximum possible current attained was on the average 35·6 per cent.; the maximum value of this percentage was 54·7 per cent. on one day, and its minimum value 11·2 per cent.

The total energy converted was 437,463 kilowatt-hours, and the amount used by consumers 355,364. The losses are thus 18·78 per cent. of the output.

On March 31, 1892, there were the equivalent of about 24,000 8-C.P. lamps on the mains.

The total capital expenditure was about £94,860, and the energy is supplied at 9d. per unit, which is considered in Germany "extraordinarily cheap," but discounts bring the average price to 8·5d. The profit on the first year's working is about £2,500.

E. L. NICHOLS—THE CARBON DEPOSIT IN GLOW-LAMP BULBS.

(*Elektrotechnische Zeitschrift*, *No.* 11, 1893, *p.* 152.)

It is well known that a glow lamp run on a constant-pressure system steadily diminishes in illuminating power, and at the same time there is an increase in the amount of energy used per candle-power—these alterations being most noticeable with new lamps—and three causes contribute to this effect: (1) diminution of the vacuum, (2) increase of resistance due to wasting of the filament, and (3) the layer of carbon particles deposited on the inner surface of the bulb. The author set himself to examine, with suitable apparatus, the deposit in three directions—viz., as regards power of absorption for different rays; as regards increase in density, and distribution; and, lastly, how far this cause accounted for the diminution in illuminating power.

As regards the first point—colour—the deposit was found to have a neutral tint, absorbing nearly the same percentage of all the rays. The deposit was found to be symmetrically distributed in a horizontal plane in twelve directions, and its variations do not amount to more than 10 per cent. or so. The following tables give the relative illuminating power, efficiency, and transparency of bulb in two representative cases:—

Table I.

Time.	Relative Brightness.	Relative Efficiency.	Relative Transparency.
0 hours	100·0	100·0	100·0
100 ,,	78·1	81·5	91·44
200 ,,	67·5	70·9	85·91
400 ,,	60·4	66·8	82·46
800 ,,	45·0	51·3	78·24

Table II.

Time.	Relative Brightness.	Relative Efficiency.	Relative Transparency.
0 hours	100·0	100·0	100·0
50 ,,	92·6	92·6	...
109 ,,	91·4	90·1	...
200 ,,	86·6	87·3	90·5
400 ,,	80·5	81·6	...
511 ,,	78·7	80·0	...
600 ,,	78·0	79·5	...
900 ,,	...	...	85·0

The influence of the deposit was found to diminish as the temperature of the filament was increased, and it is also noticeable that the diminution in brightness goes on most rapidly during the earlier stages of the life of the lamp.

ANTON ASCH—HOT-WIRE VOLTMETER BY HARTMANN BRAUN.

(*Elektrotechnische Zeitschrift*, *No.* 12, 1893, *p.* 162.)

In this voltmeter an ingenious use is made of the "sag" principle, the multiplication being doubled in the following manner:—The platinum silver wire, about 6 in. long, is stretched horizontally between two supports, and is pulled in the middle by a brass wire at right angles to it fixed at the lower end, this brass wire being pulled in its middle by a thread passing round the hand pulley to a suitable spring. The current is passed through the platinum silver wire only. The arrangement makes a compact circular voltmeter, but the hand, of course, only travels through 90°. Only 0·6 watt is spent in the platinum silver wire, so that the forces working the instrument are very small.

ANON.—THE DEVELOPMENT OF DRY PLATES BY ELECTRICITY.

(*Elektrotechnische Zeitschrift*, *No.* 12, 1893, *p.* 168.)

Dr. Liesegang, of Düsseldorf, has combined two experiments made by Schützenberger in 1869 and Eder in 1886. He passes a current from six Gassner dry cells through a concentrated solution of sodium sulphate; the zinc pole of the battery is coupled to a large platinum electrode in the outer vessel, and the positive pole is placed in a porous cell filled with the same solution. If a silver bromide plate which has had considerable exposure in the camera be placed in the vessel, a brownish-red negative image is developed in a few minutes, the solution remaining quite clear. The instability of the combination has no effect in this electrolytic method, as it is constantly being produced afresh. The picture is weak and becomes even thinner in the fixing bath.

F. J. SMITH—HIGH RESISTANCES USED IN CONNECTION WITH THE D'ARSONVAL GALVANOMETER.

(*Philosophical Magazine, No.* 214, 1893, *p.* 210.)

To save the great expense of large wire resistances the author has made experiments on non-metallic resistances and mercury jet-contacts, and has found a very reliable resistance to be one made thus: Dry plaster of Paris and electrotype plumbago are intimately mixed in suitable proportions, depending on the resistance required (equal quantities in a tube 0·4 c.m. diameter and 11·5 cm. long measured 65,000 ω), and the mixture is rammed into a glass tube having a platinum terminal; the second terminal is fixed by compression into the material, and the tube is sealed by a blow-pipe. Very finely powdered glass mixed with plumbago is used for very high resistances, but is not so manageable as plaster. A megohm made thus costs only a few shillings.

H. MOISSAN—A NEW ELECTRICAL FURNACE.

(*Philosophical Magazine, No.* 214, 1893, *p.* 313.)

This furnace is made of two carefully planed pieces of quicklime placed one under the other, the lower one having a channel for the carbon of an arc, with a cavity in the middle acting as crucible, the material to be acted on being placed directly in it, or in a carbon crucible. A hole in the upper brick enables the operator to add to the material in the crucible. With 30 amperes and 55 volts the temperature reached 2,250°; with 100 amperes and 45 volts, 2,500°; with 450 amperes and 70 volts, it reached 3,000° (approximate temperatures). With 2,500° lime, strontia, and magnesia crystallise in a few minutes. At 3,000° the quicklime melts and runs like water, carbon reduces calcic oxide and calcium is freely formed, combining with the carbon to produce a calcium carbide. Uranium oxide, which is very refractory, is reduced at once by the carbon at 3,000°. The oxides of nickel, cobalt, manganese, and chromium are reduced by carbon in a few minutes at 2,500°. Boron and silicon have also been made to act on metals, with production of beautiful borides and silicides. The investigation is being continued.

E. C. C. BALY—SEPARATION AND STRIATION OF RAREFIED GASES BY THE ELECTRIC DISCHARGE.

(*Philosophical Magazine, No.* 214, 1893, *p.* 202.)

The author of this paper has made a series of experiments on mixed and pure gases in vacuum tubes, from which he draws the conclusion that when a current is passed through a rarefied mixture of two gases a process similar to electrolysis is set up, one of the gases being separated out and collected round the negative pole, the other remaining in the tube. He found also that the formation of striæ was always the sign of the commencement of separation, and in further experiments proved that striæ are caused by the separation of the gases, and do not occur in a pure vapour or gas.

A. E. KENNELLY—A DIFFERENTIAL WATTMETER FOR USE WITH ALTERNATE CURRENTS.

(*Elektrotechnische Zeitschrift, No.* 12, 1893, *p.* 164.)

This wattmeter is designed for the purpose of measuring the efficiency of transformers. Hitherto the losses at heavy loads have involved large deflections with consequent liability to error, but in this case only the *difference* between primary and secondary watts is measured. The two wattmeters have a common axle; one measures the secondary watts (deflecting in one direction), and the other the primary. If both wattmeters be equally sensitive, the net deflection is a measure of the loss in the transformer.

To prevent mutual induction the coils of the two wattmeters are at right angles to one another. The instrument described is capable of being used for transformers up to 100 kilowatts.

D. TOMMASI—MULTITUBULAR ELECTRIC ACCUMULATORS.

(*Journal de Physique, March* 2, 1893, *p.* 130.)

This accumulator has its electrodes enclosed in a tubular envelope of metal, or of some elastic or rigid insulating material (celluloid, rubber, ebonite. &c.), having a number of small perforations.

At the centre of this sheath is fixed a core of lead, or of some suitable alloy, to serve as a conductor for the current, and to make contact with a coating of lead oxide, which is kept from disintegrating by the outer perforated sheath.

By this device, the amount of active matter is doubled in this accumulator, if compared weight for weight with other types, and in consequence the capacity of the accumulator is increased.

Charging is effected without difficulty at the rate of 5 or 6 amperes per kilogramme. Discharge may vary from 1 to 4 amperes per kilogramme of electrodes, and must be stopped when the E.M.F. falls to 1·7 volts.

In the case of a varying discharge, the Tommasi accumulator is said to safely stand from 6 to 8 amperes per kilogramme of electrodes.

The following are the electrical data of this accumulator:—

Initial electro-motive force	2·4 volts.
Capacity per kilogramme of electrode	20 ampere-hours.
Ampere-hour efficiency	95 per cent.
Watt-hour ,, ...	80 ,,

F. GERALDY—LONG-DISTANCE TELEPHONY.

(*La Lumière Electrique, Vol.* 47, *No.* 12, *p.* 562.)

The author first refers to a very long telephone line, which was erected in 1880, between Alger and Laghouat, in Algeria, and through which it was possible to speak satisfactorily. This line, however, was placed in the most favourable position, being far from other telegraph or telephone lines.

At that time no attempts were made to eliminate the effects of induction due to external sources, by employing Hughes's suggestion, *i.e.*, the use of two wires. It was habitual to use very powerful transmitters and special receivers.

Telephonic communication was next established between Bordeaux and Paris—a distance of 580 kilometres—with satisfactory results; but, as the Administration of Telegraphs was unwilling to give up two telegraph lines for telephonic purposes, matters came to a standstill. The question was revived when Van Rysselberghe devised the system of telegraphing and telephoning on one line. This was first tried in Belgium, and it was found that the best results were obtained on lines made of phosphor-bronze. This system was adopted between France and Belgium with great success. It was found that the nature of the line itself, independently of external sources, played an important part in the results obtained.

It was at this time that Mr. Preece gave the now well-known law, viz.: That the efficiency of a line for telephonic purposes depends on the product of the capacity of the line into its resistance; and according to the values of this product, the following results are obtained:—

Transmission of speech is—

Impossible	when K R =	15,000
Possible	,,	12,500
Good	,,	10,000
Very good	,,	7,500
Excellent	,,	5,000
Perfect	,,	2,500

The value of K R for the London-Paris telephone cable is 7,500, which should mean a very good result, but is actually almost perfect.

In America experiments have been made of late on lines 1,000, 1,500, 1,900 kilometres long. The above formula showed that great weights of wire would be necessary for these lines. Actual experiments were made which yielded the following results—

Transmission of speech was—

Impossible	through	1,750	kilometres	when K R =	... 94,000
Fair	,,	1,420	,,	,,	... 62,000
Good	,,	1,200	,,	,,	... 45,000
Excellent	,,	1,000	,,	,,	... 31,000

A line was consequently erected between New York and Chicago with K R = 32,000, and very satisfactory results were obtained,

The author considers that the difference between the above results may be due, perhaps, to taking different values for K, for this might mean the total capacity between lines and earth, or between the two lines. Mr. Preece takes the value of K in the last case as one-half of that between lines and earth. The Americans take this, however, as 0·6, which may account for the want of concordance in the above results.

Mr. Preece states that K cannot be measured directly: divers corrections are necessary; and, in fact, the K of each long circuit must be measured by comparison with an empirical K R scale.

The author considers that if this quantity is not to be measured directly, and can only be obtained by the use of an empirical scale, it can be scarcely called by the precise term "capacity," but should be given a less arbitrary name. The author further suggests that the self-induction of the line should be taken into account. Telephone currents have a varying periodicity, but have a mean value of several hundred periods per second; and, as is well known, the effects of self-induction increase, and the effects of capacity diminish, with an increased periodicity.

With a complex overhead and underground line a true determination of capacity would no doubt be difficult to make; but perhaps, by considering the exact conditions of working, the mean periodicity, and the intensity of the currents, more concordant results might be obtained.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Journals during the Month of APRIL, 1893.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

F. Uppenborn—The Electric Central Stations of Schuckert & Co.: III.—Düsseldorf.—*E. T. Z.*, 1893, No. 14, p. 185 (S. I.).

C. Heim—Small Arc Lamps and Gas Glow Light.—*E. T. Z.*, 1893, No. 14. p. 196 (I.).

— von Reymond-Schiller—Calculation of the Strength of Batteries for Accumulator Tram-Cars.—*E. T. Z.*, 1893, No. 14, p. 201.

Anon.—The Fürstenfeld-Brück Transmission.—*E. T. Z.*, 1893, No. 16, p. 221 (I.).

G. Richard—Incandescent Lamps.—*Lum. El.*, vol. 47, No. 13, p. 618 (S. I.).

G. Richard—The Mechanical Applications of Electricity.—*Lum. El.*, vol. 48, No. 14, p. 10, No. 17, p. 158 (S. I.).

F. Géraldy—On the History of Transmission of Force.—*Lum. El.*, vol. 48. No. 15, p. 51.

Anon.—The Liverpool Overhead Railway.—*Lum. El.*, vol. 48, No. 15, p. 66, No. 17, p. 171 (S. I.).

C. Hering—The most Economical Life for Incandescent Lamps.—*Lum. El.*, vol. 48, No. 15, p. 77.

Anon.—Indirect Illumination.—*Id.*, p. 79.

Anon.—The Christiania Central Station.—*Id.*, p. 80.

J. P. Anney—The Distribution of Electric Energy.—*Lum. El.*, vol. 48, No. 16, p. 101 (S. I.).

C. Brown—A New Alternate-Current Motor.—*Id.*, p. 113.

G. Richard—Electric Tram and Railways.—*Id.*, p. 115 (S. I.).

Anon—Welding by means of the Arc.—*Id.*, p. 126.

— Brown—Method of Diminishing the Current on Starting Alternate-Current Motors.—*Id.*, p. 128.

F. Géraldy—The Lighting of Paris: Champs-Elysées District.—*Lum. El.*, vol. 48. No. 17, p. 151 (I.).

DYNAMO AND MOTOR DESIGN.

G. Schilling—On Alternate-Current Motors.—*Beibl.*, 1893, No. 4, p 364.

P. Hoho—Compound-Wound Dynamos.—*C. R.*, vol. 116, No. 15, p. 744.

G. Richard—Details of Dynamo Construction.—*Lum. El.*, vol. 48, No. 15, p 58 (S. I.).

ACCUMULATORS.

ANON.—Pollak's Method of Charging Accumulators with Alternate Currents.—*E. T. Z.*, 1893, No. 15, p. 218.

E. PEYRUSSON—A Secondary Cell.—*Bull. Soc. Int.*, 1893, No. 97, p. 189.

ANON.—The Robins Accummulator.—*Lum. El.*, vol. 48, No. 14, p. 24 (I.).

ANON.—The Harris Accummulator.—*Id.*, p. 25 (I.).

ANON.—The Andreoli Cell.—*Id.*, p. 26.

ANON.—The Urquhart and Small Cell.—*Id.*, p. 27 (I.).

ANON.—The Heyl Cell.—*Id.*, p. 27 (I.).

ANON.—The Edgerton Cell.—*Id.*, No. 17, p. 179 (I.).

MAGNETISM.

A. BANTI—Influence of Mechanical Disturbances on the Magnetisation of Nickels.—*Beibl.*, 1893, No. 4, p. 357.

— CANTONE—Influence of Transverse Magnetisation on the Resistance of Longitudinally Magnetised Iron and Nickel.—*Beibl.*, 1893, No. 4, p. 358.

A. BERGET—On the Magnetic Expansion of Iron.—*Jour. de Phys.*, 1893 (April), p. 172 (I.).

MOORE and TINGLEY—Curves of Magnetisation of Iron by Alternate Currents.—*Lum. El.*, vol. 47, No. 13, p. 626 (I.).

INSTRUMENTS AND MEASUREMENTS.

C. CHRISTIANSEN—A New Electrometer.—*W. A.*, 1893, No. 4, p. 726.

H. E. DU BOIS—A Magnetic Balance and its Use.—*Beibl.*, 1893, No. 4, p. 353.

ANON.—Galvanoscope of the Allgemeine Elektricitäts Gesellschaft.—*Beibl.*, 1893, No. 4, p. 354.

— HENRION—A Registering Voltmeter.—*Beibl.*, 1893, No. 4, p. 354.

J. W. GILTAY—An Electro-Dynamometer for Measuring Telephone Currents.—*Beibl.*, 1893, No. 4, p. 354.

ANON.—A New Arrangement of Wheatstone's Bridge.—*E. T. Z.*, 1893, No. 14, p. 207 (I.).

R. M. FRIESE—A Mirror Wattmeter.—*E. T. Z.*, 1893, No. 15, p. 209 (S. I.).

H. SESEMANN—A Telegraphic Transmitter.—*E. T. Z.*, 1893, No. 15, p. 212 (I.).

N. H. GENUNG—Improvements in the D'Arsonval Galvanometer.—*E. T. Z.*, 1893, No. 15, p. 212 (I.).

A. BLONDEL—The General Principles of Registering and Indicating Instruments.—*C. R.*, vol. 116, No. 15, p. 748.

— MOORE—Magnetometric Method of Measuring Iron Losses under Alternating Currents.—*Lum. El.*, vol. 47, No. 13, p. 626 (I.).

F. GUILBERT—Electric Railway Signalling.—*Lum. El.*, vol. 48, No. 14, p. 19 (I.).

ANON.—The Willyoung Electro-thermal Ammeter.—*Lum. El.*, vol. 48, No. 14, p. 24 (I.).

ANON.—The Ferranti Meter, 1892.—*Id.*, p. 28 (I.).

Anon.—The Weston Ammeter, 1893.—*Id.*, p. 31 (I.).

— Abraham—A New Determination of "*v*."—*Lum. El.*, vol. 48, No. 14, p. 38 (I.).

ELECTRO-CHEMISTRY.

G. H. Zahn—On Phenomena occurring at the Junction of Solutions of different Strengths with the Passage of an Electric Current.—*W. A.*, 1893, No. 4, p. 606.

R. Malagoli—Electrolysis by means of Alternate Currents.—*Lum. El.*, vol. 47, No. 13, p. 610 (S. I.).

Anon.—The Formation by Electrolysis of Porous Lead for Accumulators.—*Lum. El.*, vol. 48, No. 15, p. 76 (I.).

Anon.—The Manufacture of Electrolytic Chlorine (Cutten Process).—*Lum. El.*, vol. 48, No. 16, p. 124 (I.).

TELEGRAPHY AND TELEPHONY.

— Gansauge—Static Induction in Overhead Wires.—*Jour. Tel.*, vol. 17, No. 4, p. 73.

Anon.—Improvements in French Telegraph and Telephone Services.—*Jour. Tel.*, vol. 17, No. 4, p. 78 (S.).

Anon.—Telegraphic Statistics, 1891.—*Jour. Tel.*, vol. 17, No. 4, p. 82.

Anon.—Scribner's Trial Circuit for Multiple Telephone Switch-Board.—*Lum. El.*, vol. 47, No. 13, p. 624 (I.).

Anon.—The Forbes Telephone.—*Lum. El.*, vol. 48, No. 14, p. 25 (I.).

Anon.—The Derant Semaphore.—*Id.*, p. 26 (I.).

Anon.—The Francis Telephone Hook.—*Id.*, p. 32 (I.).

Anon.—The Marinowitch and Szardair Telephonic Distribution.—*Lum. El.*, vol. 48, No. 17, p. 177 (I.).

ATMOSPHERIC AND STATIC ELECTRICITY.

H. Ebert and E. Wiedemann.—On Electric Discharges: Production of Electric Oscillations, and the Relation of Discharge Tubes to them.—*W. A.*, 1893, No. 4, p. 549.

E. Goldstein—A Property of the Anode in Geissler Tubes.—*W. A.*, 1893, No. 4, p. 785.

E. Goldstein—On an Apparent Repulsion between similarly directed Cathode Rays.—*W. A.*, 1893, No. 4, p. 787.

E. Branly—Loss of Charge in Diffused Light and in Darkness.—*C. R.*, vol. 116, No. 15, p. 741.

— Birkeland—On the Reflection of Electric Waves at the Extremity of a Linear Conductor.—*C. R.*, vol. 116, No. 16, p. 803.

W. de Fonvielle—Measurements of Earth Currents at the St. Maur Observatory.—*Lum. El.*, vol. 48, No. 14, p. 8 (I.).

A. RIGHI—Distribution of Potential in an Electric Field in Rarefied Air.—*Lum. El.*, vol. 48, No. 17, p. 192 (I.).

THEORY.

V. BJERKNES—The Penetration of Electric Waves into Metals, and the Electric Theory of Light.—*W. A.*, 1893, No. 4, p. 592.

H. VON HELMHOLTZ—Appendix and Corrections to the Paper on the Electromagnetic Theory of Diffraction.—*W. A.*, 1893, No. 4, p. 723.

D. A. GOLDHAMMER—On the Electric Theory of Magneto-optical Phenomena.—*W. A.*, 1893, No. 4, p. 740.

A. OBERBECK—On the Behaviour of Colloidal Silver towards the Electric Current. (Reply and Corrections.)—W. A., 1893, No. 4, p. 745.

E. BELTRAMI—Observations on the Mathematical Theory of Magnetism.—*Beibl.*, 1893, No. 4, p. 353.

G. FITZGERALD—Electro-magnetic Radiation.—*Beibl.*, 1893, No. 4, p. 359.

G. M. MINCHIN—The Magnetic Field of a Circular Current.—*Phil. Mag.*, 1893, No. 215, p. 354.

Dr. BEHN-ESCHENBURG—The Method of Working of Alternate-Current Motors.—*E. T. Z.*, 1893, No. 14, p. 203.

A. HESS—On Heterogeneous Dielectrics.—*Jour. de Phys.*, 1893 (April), p. 145 (I.).

BEDELL and A. CREHERE—Effect of Self-Induction and Capacity distributed along a Conductor.—*Jour. de Phys.*, 1893 (April), p. 191.

C. E. GUILLEAUME—Modern Ideas on the Theory of Electric Dimensions.—*Bull. Soc. Int.*, 1893, No. 97, p. 185.

E. MERCADIER—On the General Relations between the Coefficients and Fundamental Laws of Electricity and of Magnetism.—*C. R.*, vol. 116, No. 16, p. 800.

E. MERCADIER—Rational Systems of Expression in Dimensions of the Electric and Magnetic Quantities.—*C. R.*, vol. 116, No. 17, p. 872.

VARIOUS.

— MOSELEY—Preparation of Zinc Electrodes for Galvanic Cells.—*Beibl.*, 1893, No. 4, p. 350.

— GERMAIN—A Galvanic Cell with Cellulose Filling.—*Beibl.*, 1893, No. 4, p. 350.

— MÜLLER—On the Behaviour of the Zinc Electrode in the Braunstein Cell.—*Beibl.*, 1893, No. 4, p. 351.

M. MÜTHEL—Filling for Zinc Carbon Cells.—*Beibl.*, 1893, No. 4, p. 352.

D. INFREVILLE—A Primary Cell.—*Beibl.*, 1893, No. 4, p. 352.

P. JANET—Some Researches with High-Frequency Alternating Currents.—*Beibl.*, 1893, No. 4, p. 360.

E. DORN—Phenomena produced in Rarefied Gases under the Influence of very Rapid Electric Oscillations.—*Beibl.*, 1893, No. 4, p. 361.

J. VIOLLE—On the Temperature of the Electric Arc.—*Beibl.*, 1893, No. 4, p. 363.

E. Lagrange and P. Hoho—Phenomena of Light and Heat produced in Liquids by Electric Currents.—*Beibl.*, 1893, No. 4, p. 362.

C. Thwing—A Photographic Method of presenting a Magnetic Field.—*Jour. de Phys.*, 1893 (April), p. 191.

D. Korda—Multiplication of Number of Periods of Sinusoidal Currents.—*C. R.*, vol. 116, No. 16, p. 806.

A. Ditte—Contribution to the Theory of the Leclanché Cell.—*C. R.*, vol. 116, No. 16, p. 812.

D. Korda—Measurement of the Difference of Phase between Two Sinusoidal Currents.—*C. R.*, vol. 116, No. 17, p. 876.

H. Rigollot—Effect of Colouring Matters on Actino-electric Phenomena.—*C. R.*, vol. 116, No. 17, p. 878.

E. Brylinski—On the Electrification of Gutta-Percha.—*Lum. El.*, vol. 47, No. 13, p. 601.

Anon.—Bentley Pneumatic Cut-out.—*Lum. El.*, vol. 47, No. 13, p. 624 (I.).

W. E. Ayrton—On Dr. Fleming's Transformer Experiments.—*Lum. El.*, vol. 48, No. 14, p. 21.

Anon.—The Rend Medical Battery.—*Lum. El.*, vol. 48, No. 14, p. 24 (I.).

Anon.—The Davis Cell.—*Lum. El.*, vol. 48, No. 14, p. 25 (I.).

Anon.—Fittings for High-Tension Circuits (Ferranti).—*Id.*, p. 28 (I.)

Anon.—The Craney Electrolyser.—*Id.*, p. 31 (I.).

Anon.—The Laurent-Cely and Finot Primary Battery.—*Id.*, p. 31.

L. Morisse—American Gutta-Percha.—*Lum. El.*, vol. 48, No. 15, p. 19, No. 16, p 139 (S.).

L. Neustadt—Phenomena observed in Concentric Cables with Alternate Currents.—*Lum. El.*, vol. 48, No. 16, p. 119 (I.).

P. Marcillac—Contribution to the Study of Alternating Currents.—*Id.*, p. 121 (I.).

A. Hess—Gray's Telautograph.—*Lum. El.*, vol. 48, No. 17, p. 167 (I.).

Anon.—The Johnston Insulator.—*Id.*, p. 179 (I.).

Anon.—Wurt's Thermal Cut-out.—*Id.*, p. 179 (I.).

Anon.—Wurt's Lightning Arrester.—*Id.*, p. 180 (I.).

Anon.—Kenjon-Wilson Block System.—*Id.*, p. 180 (I.).

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS.—No. 107.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII.	1893.	No. 107.

The Two Hundred and Fifty-third Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 11th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 27th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Augustus Archer.	Henry Harold Perry.
Horace L. P. Boot.	A. J. Protheroe.
Walter Hawkings.	Louis Heathcote Walter.
Arthur Everard Levin.	Norman Whichello.

Mr. J. S. Fairfax and Mr. J. O. Girdlestone were appointed scrutineers of the ballot.

The PRESIDENT: Gentlemen,—My first task is perhaps one of the most unfortunate and painful that the President of an Institution of this kind can possibly discharge: it is, to allude to the ill effects which Time inflicts on all communities when his scythe sweeps around and about us. The Council have had announced to them this evening the death of three prominent members of this Institution. But there is one who has gone to his long rest whom many of us knew intimately, and whose loss will be very severely felt by a large class of members of this Institution. I refer to Sir James Anderson—a man whom I have known intimately for nearly thirty years. As a man, I have never met anybody who carried so completely in his face those characteristics of pure manliness which he undoubtedly possessed. Whether you met him as an officer on the bridge of his ship when he was a distinguished captain in the Cunard service, or whether you met him on that gigantic vessel the "Great Eastern" when he successfully laid the cable across the Atlantic,—whether you met him as a director of the company which he managed so well, or whether he appeared amongst us here,—he was always one who succeeded in winding himself around your heart, and you felt that in him you had a true friend and a strong man. In addition to this he was essentially a gentleman; and I am sure that all the members of the Eastern Telegraph Company and its associated companies, who have acted in various capacities under him, will say that they never could wish to have a chief who looked after their interests, who carried their secrets and attended to their wants more faithfully or more honestly than he did. The Council this evening have desired that a vote of condolence be sent to his son as the representative of the family. and I am quite sure that I am only echoing the sentiments of all the members of this Institution when I say that they heartily join the Council in this act of sympathy with his family, and of reminiscences of the services which the Institution has received from Sir James Anderson.

The meeting unanimously signified their acquiescence.

The following paper was then read:—

ON THE PREVENTION AND CONTROL OF SPARKING; CONTINUOUS-CURRENT DYNAMOS WITHOUT WINDING ON THE FIELD MAGNETS, AND CONSTANT-PRESSURE DYNAMOS WITHOUT SERIES WINDING.

By W. B. SAYERS, Associate.

The object of this paper is chiefly to lay before the Institution certain means for bringing under independent control the commutation of ring and drum armatures for continuous currents. The limits dependent on considerations of sparking at the brushes being thereby done away with, a new field of design is opened up. Mr. Sayers.

Mr. Swinburne and Mr. Esson—the former in his paper read before this Institution in 1890, and the latter in his papers in 1890 and 1891—both give expressions for the maximum load which can be carried without sparking; these expressions are in terms of the ampere-turns upon the armature, the length of the air space, the angle subtended by the polar surfaces of the field magnets, and the forward induction. Thus it is recognised that in ordinary ring and drum armature machines the consideration of sparking fixes, or at least limits in one direction, some of the most important elements in the design of the machine, and is not separately controllable. A practical result from this is that the lightening of machines by putting the conductors in tunnels, reducing the air space to a mere clearance—which has been proposed from time to time, notably by Mr. Swinburne, and which is the condition in which minimum exciting force is required, and, consequently, the condition of minimum weight of magnets—has not been hitherto practicable.

The conditions of sparkless collection, when no special means are employed for bringing about the reversal of the sections as they pass the diameter of commutation, are now well understood; but as I have found it somewhat difficult to describe the action of the devices which I wish more particularly to bring under consideration, I have prepared four diagrams which I think represent the action fairly well, and which will at least be useful

Mr. Sayers. as a stepping-stone towards that which I wish to describe later on.

In diagrams 1 to 8 I have ventured to distinguish the currents flowing from the two halves of the armatures by red and blue colouring* beside the wires in which they are flowing. This is merely to enable me to refer briefly to the two halves of the current, and to clearly distinguish them in the diagrams.

Diagrams 1 to 4 represent part of an ordinary ring armature machine spread out flat. Suppose, as is the case usually, the brush makes contact over an angular breadth equal to that of one section of the commutator, or a little more: then the current reaches the brush alternately through one commutator bar, and through two. Figs. 1 and 3 represent diagrammatically these two conditions. In Fig. 1 the commutator bar C is represented centrally under the brush. In this position the currents flowing from the two halves of the armature unite in the commutator connection B, and flow thence through the commutator bar to the brush. As the armature moves onward in the direction indicated by the arrow, the armature coil A is seen to be short-circuited under the brush. The current flowing in it at the moment when it becomes short-circuited would be counter clock-wise, looking from the lower end; but the coil, from its position, is beginning to cut lines of force in a sense tending to induce a clock-wise current. The counter clock-wise current therefore falls, and at the same time a current rises in B^1, such that the current in B, plus the current in A, equals at any moment the half-armature current. Fig. 2 represents the position when the red half is equally divided between B and B^1, B carrying in addition the whole of the blue half. The next stage is shown in Fig. 3, when the current in the armature coil A is passing through zero value; at this instant the blue half of current flows to the brush by B alone, and the red half by B^1 alone. A clock-wise current now commences to rise in A, the current in B falling correspondingly. Fig. 4 shows the position when the blue half is equally divided between B and B^1, while

* In the blocks ——— stands for red colouring, and —·—·— stands for blue colouring.

Mr. Sayers

B^1 carries in addition the whole of the red half. As the armature still moves on, the position shown in Fig. 1 is again reached, the current in A having risen, under the influence of the induction, to the value of half the armature current. At the instant when this is so, the current in B^2, Fig. 1 (which we now look upon as B moved through the angular distance of one section), is necessarily zero, and if the tip of the brush breaks connection with the commutator at this instant the break will be sparkless.

Several devices have from time to time been proposed for controlling sparking at the brushes of dynamo machines—notably the use of reversing pole-pieces. Mr. Hookham, of Birmingham, told me—I think in 1889—that he had succeeded in obviating the necessity of moving the brushes for varying loads by their means. Mr. Swinburne has also advocated the use of reversing pole-pieces; and Mr. Edison has from time to time since 1883 patented various arrangements with the object of introducing a counter electro-motive force into the short-circuited coil, with the object of avoiding sparking and the necessity for moving the brushes with varying loads.

Still, there would appear to be in the minds of some a vague idea that sparking is due generally to an abnormal current being generated in the coil which is short-circuited under the brush. That is far from being true as a general rule, as appears from the actions I have described. An excessive current can only be reached in the short-circuited coil after the requisite reversal has taken place. Take Fig. 1. If the brush does not break at the instant here depicted, the current in the armature coil A^2 will rise above the value of half-armature current, a back current being started in B^2 which flows round through A^2 and B to the brush, and may be considered as superimposed upon the half-armature current in A^2 and B; when the break occurs, there will therefore be a spark, which, it may be noted, will tend to maintain itself as an arc across the insulation between C and C^2. This excess current in A^2 and B, then, only occurs after the reversal of the section has been duly accomplished—that is, when the reversing of the section has been carried too far. Now moving the brushes back

Mr. Sayers. so that the coil is short-circuited in a slightly weaker field would at once correct this fault. We must, therefore, conclude that if with a given load there is no sparkless position for the brushes of a machine, it is because the coil which has been short-circuited under the brush comes mechanically into the circuit with too little or no current in it, or, in a very bad case, even with the current still in the wrong direction; in other words, the reversal has only been partially effected, or not effected at all, by the inductive action of the field. If in an ordinary machine (speaking of generators) the brushes are shifted backwards so that the armature coils are short-circuited while cutting lines of force of such direction as not to tend to reverse the current in them at all, but rather to increase it, the reversal is only accomplished by the mechanical make-and-break action of the brushes and commutator sections, and is consequently accompanied with violent sparking. I think it is abundantly clear, then, that excess of current in the coil which is commutated is never in practical work the cause of sparking, but that, on the other hand, sparking which cannot be got rid of by shifting the brushes forward is due to the current of the coil being too small when the break occurs between the commutator section and the brush.

It is still the practice with some makers to use driving pins at intervals, causing a greater angular space between some of the armature sections than the normal, and the consequence is that the brushes cannot be in the correct position for all the armature coils at the same time. Such machines will invariably be found to be marked at the commutator in a manner regularly increasing and decreasing in relation to the position of the driving pins: *the marking will be more or less distinct according to the nearness to the same position in which the brushes are set from time to time for the same loads;* because, if in such machines the brush is shifted through a small angle, the sparking is transferred from some sections to others, so that if the brushes are moved about sufficiently the sections will all get marked alike. When the pins are necessary there seems to me to be a very simple remedy for the defect under consideration. I suggested

this remedy to Mr. Crompton about 18 months ago, but I do not know whether he has made any experiments with it. The remedy is to make the divisions in the commutator to correspond with the divisions of the armature sections. Thus, in the position corresponding to the position of a driving pin there would be a commutator bar having an angular width equal to the normal width of a section, plus the angular width occupied by the driving pin. For good appearance the width of pin might be equal to the angular width of the commutator section, and a dummy section connected to one of its neighbours put in the commutator at the position corresponding to the position of the driving pin. Mr. Sayers.

Before describing the special means by which I secure the sparkless reversal of the section under the brush at any desired place between the horns of the pole-pieces, I may briefly state the characteristic features of the new machines which the use of the devices referred to has enabled me to design. These are as follows:—The air space is a mere clearance—1 millimetre. The reversal of the sections is effected by inductors, or coils, which I have called "commutator coils," independent of the main winding. These commutator coils are not inserted in the closed or re-entrant circuit of the ring, or drum. On the contrary, they are inserted in the connections that run at intervals from the re-entrant winding to the several bars of the commutator. Each such coil comes, therefore, into operation only intermittently, once in each semi-revolution. The function of these coils is to furnish electro-motive forces that will balance those due to back-induction and self-induction in the sections as they are successively reversed. These commutator coils are so arranged as to be acted on by the pole-tip which is strengthened by the armature current, and the brushes of the machines when run as generators are set with a backward lead instead of a forward one. The commutator coils enabling me to reverse the armature sections just after they have emerged from under the strengthened pole, the result is that what have hitherto been called back-turns of the armature winding become forward-turns, and the effect of the cross induction is to increase the reversing field instead of to diminish it.

Mr. Sayers. The machine illustrated in diagram No. 9 has all of these features. It is self-exciting by means of the armature winding only—that is, it generates a current—behaving like a series machine, of course—without any winding at all upon the magnets, which may, therefore, more properly be called keepers; and it runs absolutely without spark at the brushes.

Before further describing this particular machine, I will describe as clearly as I can the means which I adopt for obtaining these results.

Figs. 5 to 8 represent diagrammatically a ring armature machine wound with "commutator coils" for causing the reversal of the armature sections close against the horn which is strengthened by the armature reaction. The commutator coils consist of coils wound around the armature core, and each connected at one end to the main winding at points between the sections, and at the other end to the commutator segments. There are thus as many commutator coils as there are sections in the armature. The commutator coils are placed at an angular distance from the sections of the winding between which they are connected, being from one to two sections nearer the pole horn which is strengthened by the armature current. The direction of winding of the commutator coils relatively to the main winding is such that if the winding is followed around the core from a point in the main winding under the "flux" horn—which I have ventured to name the horn which is strengthened by the armature current—in the direction which leads out from under the tip of the horn to a point where a commutator coil branches off, and the commutator coil is then followed, the wire leading to, and forming, the commutator coil doubles back under the flux horn and is then wound around the core in the same direction as that followed when tracing the main winding. This will be seen clearly on referring to Fig. 5. Starting from the point E and following the main winding down to the point D, then passing up commutator coil B, it will be seen to pass around the core in the same direction as the main winding.

Figs. 5, 6, 7, and 8 represent four stages in the commutation of an armature by means of my commutator coils, corresponding

to the four stages represented in respect of ordinary winding in Figs. 1, 2, 3, and 4. Mr. Sayers.

In the following description it will be seen that the actions depend essentially upon the difference of pressure induced in the two or more commutator coils concerned at any time, and it follows that the angular distance between the coils must be considerably greater than the length of the air space, otherwise the difference between the E.M.F. generated in the two bars may be insufficient. This necessity of course fits admirably with the conditions in short air space machines. I have, indeed, made no experiments up to the present with the device on smooth-core armatures, but when used on such machines I think it would be necessary to *reduce* the number of sections in the armature to less than the usual practice, so as to get a sufficient distance between the two commutator coils or bars to produce the required preponderance of pressure in one of the two or more segments under the brush.

Fig. 5 illustrates the approximate conditions when one commutator plate is central under the brush. The red half of the armature current meets the blue half at the point D, whence the whole current passes by the commutator coil B to the brush; and it will be noted that as the coil B is just under the tip of the pole horn, the E.M.F. due to one turn is generated in it, and is added to that generated in the main winding. This being the case at the other brush also, the effect of the commutator coils in increasing the E.M.F. is equal to that produced by four of the main conductors on the periphery of the armature. Fig. 6 represents the armature a quarter section further on than Fig. 5. The commutator coil B having moved away from the pole-tip, and consequently into a weaker field, has less E.M.F. generated in it; there is also a fall of potential throughout its length due to the resistance of the conductor. The commutator coil B^1, however, which is now in contact with the brush, is still under the pole, and B and B^1 are united at their inner ends by the main winding, section A. Now the main winding, section A, although yet a considerable distance from the neutral zone, is cutting few lines of force compared with a coil under the flux horn. Under these

Mr. Sayers. conditions, then, the current which was flowing in B is transferred to B^1 under the action of the resultant E.M.F. in the circuit composed of armature coil A and commutator coils B and B^1 completed by the brush.

Fig. 6 shows the instantaneous condition a quarter section further on than that shown in Fig. 5, half the red half of armature current flowing through B^1, the other half flowing through the armature coil A and joining the blue half at D, and thence through B to the commutator.

Fig. 7 shows the point half a section from the starting point depicted in Fig. 5, where the current in the armature, section A, is at zero value, the red half of armature current flowing out by B^1 and the blue half by B.

Fig. 8 shows a point three-quarters of a section from the starting position, when half the blue half is still flowing to the brush *via* B, while the other half has commenced to flow around A (thus starting the current in the required direction in A); arriving at the point D, it joins the red half and flows through B^1 to the brush. Fig. 5 may be regarded as the next stage, the armature having moved through an angular distance equal to one section; the current in B^2 (which we regard as B moved one section on) is almost extinct, A is carrying nearly the normal = half armature current, and by the time the commutator segment breaks with the tip of the brush the current flowing from it will be zero, and the break consequently sparkless.

Proceeding now to consider practical applications of my device, Fig 9 represents diagrammatically the first machine made self-exciting by means of the armature winding alone. The machine is now running at the Bradford Station Hotel, Midland Railway, as a regulator, in accordance with an arrangement designed by my brother, Mr. J. Sayers. It is coupled direct to a shunt motor, and driven at constant speed. The main current to the hotel is passed through the armature, causing an induction proportional to itself, and thus raising the pressure in proportion to the current flowing, and compensating for the drop in the leads, just as a series machine would do under the same conditions.

W. B. Sayers.

Fig 3.

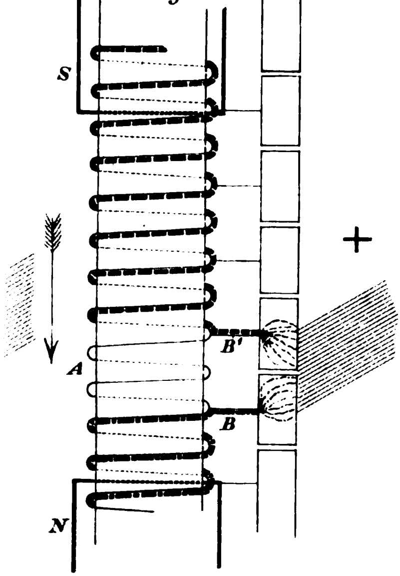


Fig 4.

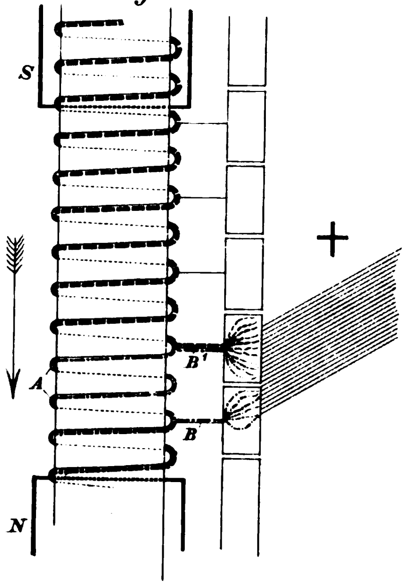


Fig 7.

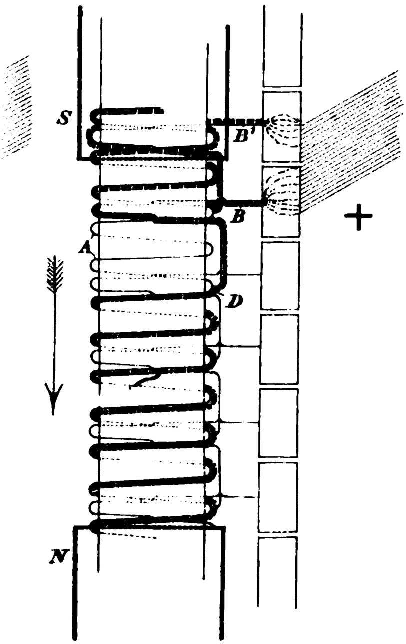
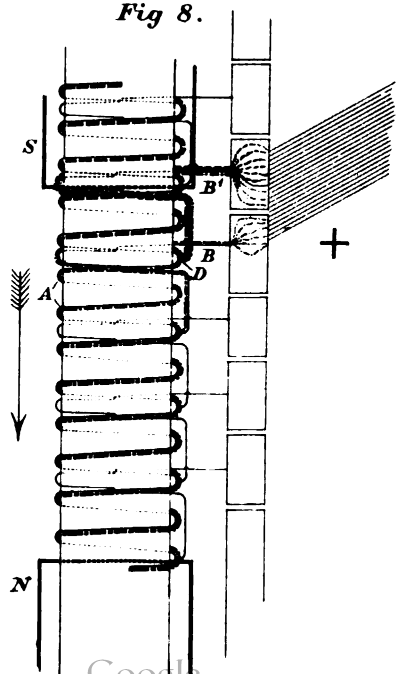


Fig 8.



Particulars and dimensions of the machine are as follows:— Mr. Sayers.

Armature—Ring form.

Diameter, external	... ...	$9\frac{5}{8}$ in.
„ internal	... ...	$4\frac{1}{4}$ „
Length ...	... ...	9 „

The periphery has 40 slots cut into it in which the conductors are placed. The slots are $\frac{3}{4}$ in. deep, $\frac{1}{4}$ in. wide, and each has two grooves cut in it opposite to each other near the top. The main winding conductor is of copper strip 0·15 × 0·5 in., and occupies the space in the slot below the grooves. The commutator coils or bars are of copper 0·1 in. × 0·3 in., with rounded edges, insulated D.C.C., then double-lap tape. They are slid into the slot from one end, their edges fitting into the two grooves. Thus they serve to hold the conductors in position, and so obviate the necessity for binding. The air space is 1 millemetre.

The keepers (they really are not field magnets) are of double L shape, and made up of charcoal iron plates to prevent heating from the variation of the flux over the polar surface caused by the slots in the armature. The keepers are separated in the centre in order to reduce the cross induction, which is of course very considerable.

I have had in mind trying Professor S. P. Thompson's suggestion of making up the keepers—or field-magnets, as the case may be—of separate plates, with the object of reducing cross induction. In subsequent machines the necessity for laminating the polar surfaces has been obviated by putting the conductors into tunnels, and only running a saw-drift through the crown of the tunnel to break the iron circuit and reduce the self-induction of the section; the well-known device of winding over with charcoal iron wire has also been used.

On testing the machine illustrated in Fig. 9, it was found, as I expected, to be self-exciting, and it will carry a current of 400 amperes at 14 volts running at 900 revolutions per minute. It is capable of running with this current absolutely without spark at the brushes. There is hardly any external field—sufficient, of course, to move a compass, but hardly enough to feel

Mr. Sayers. with a piece of iron—except between the two keepers in the space marked F, where there was considerable field, due, of course, to cross induction.

According to Mr. Esson's formula, the output should be $(20{\cdot}5)^2 \times 22{\cdot}5 \times 15 \times 0{\cdot}048 = 7{,}000$ watts. The comparatively small output obtained—5,600 watts—is accounted for by the fact that for the purpose for which it was designed (to act as a regulator) it was necessary to keep the induction density in the iron below 10,000 in order that the E.M.F. should be as nearly as possible proportional to the current.

Fig. 10 is the characteristic curve of the machine—the

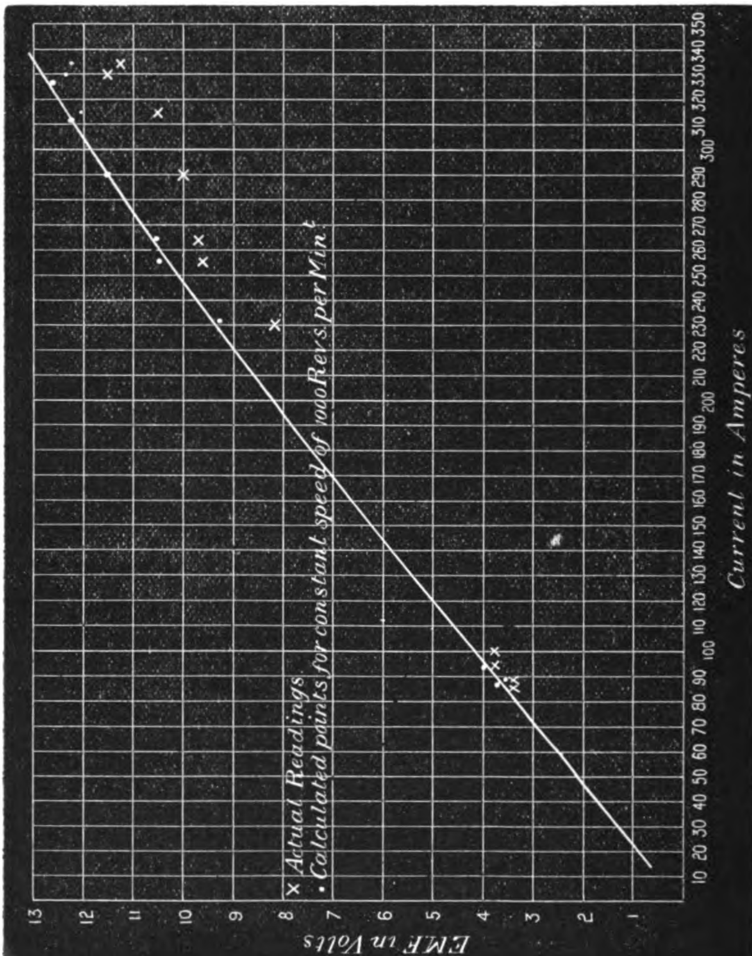


Fig. 10.

horizontal distances representing the current, the vertical distances the volts. Mr. Sayers.

The most remarkable feature observed about the machine when self-exciting is its extreme sensitiveness with regard to the position of the brushes, a very small movement causing a great variation in the volts. Moreover, it will not excite at all unless the brushes are within a certain arc of about 9 degrees; the position of this arc corresponding with the position in which the hindermost commutator bar under the brush is under the pole-tip, and the foremost one has passed out from under it.

The curves to the right of the brush in Fig. 9 represent the volts observed with the brushes in various positions. The pointer drawn through the brush indicates the point in the curve corresponding with the brushes in the position shown. Thus, if the brush and pointer were shifted in either direction, the pointer would indicate the volts corresponding with the position of the brush.

Curve No. 1 is plotted from results obtained when the machine was running at a speed of 850 revolutions per minute, and an external resistance in circuit of about 0·03 ohm, the maximum current reached being about 340 amperes. For curve 2 the speed was 1,000 revolutions per minute; external resistance, 0·038 ohm; and maximum current, 291 amperes. Curve 3 shows the effect of one turn of series winding around each keeper, with speed 1,000; external resistance, 0·0455 ohm; current, 202-265 amperes. Curve 4 shows the effect of moving the brushes when a constant current is passed through the armature: speed, 1,000; current, 200 amperes. These curves are plotted from the tables I., II., III., IV., given below.

It will at once be obvious to all that a shunt machine with armature on my principle may be made to keep pressure constant with varying current, part of the armature winding performing the function hitherto performed by the use of series coils on the magnets. As would be expected, the armature coils so used are several times more effective, turn for turn, than an equal number of series coils upon the magnets. First, they count double, because they would exist as *back-*

Mr. Sayers. turns if the machines were running with a forward lead; and, second, because, whereas in the latter case the effect of the series coils—or, rather, of the remainder of them after subtracting the number of the back-turns—depends upon the permeability of the field magnets and armature, and all magnetic leakage is of course ineffective, the action of the forward-turns on the armature when running with a backward lead, though depending on permeability of the armature core and magnets, is wholly effective, even the leakage (if we so call the lines which do not pass around the magnet cores) being necessarily cut by the conductors on the armature.

Particulars of such a machine are as follows:—

PARTICULARS OF 9-UNIT SHUNT-WOUND DYNAMO; 100 VOLTS, AT 920 REVS. PER MINUTE.

Armature— Diameter at bottom of slots, $6\frac{7}{8}$ inches.
" of hole in centre of discs, 2 inches.
" over all, 9 inches.

Length of iron core, 10 inches.

Dimensions of slots, $1\frac{1}{16}$ inches deep by $\frac{1}{4}$ inch wide.

Number of slots, 40.

Total turns of main conductor on armature, 80.

Turns per section of armature, 2.

Commutator Coils: Turns to each section of armature, 1.

Sectional area of main armature conductor, $0·180 \times 0·160 = 0·0288$ square inch.

Sectional area of commutator coils, $0·180 \times 0·08 = 0·0144$ square inch.

Resistance of main conductor between points of connection to commutator coils at T. = 100° Fah., 0·025 ohm.

Resistance of two commutator coils in series, being the maximum resistance added by these coils to armature, at T. = 100° Fah. = 0·005 ohm.

Total resistance of armature, 0·03 ohm.

Mr. Sayers.

Weight of main armature conductor	35·4 lbs.	
„ of commutator coils ...	8·8	„
Total weight of armature conductor	44·2	„

Commutator coils one section in rear when running as generator of armature sections to which they are connected.

Field Magnets—Cores of scrap iron, 6 inches thick by 10 inches broad. Cast 3 inches into sole plate at lower ends. Sole plate therefore acts as yoke, and has approximately double the sectional area of cores.

Bore of pole-pieces, $9\frac{7}{16}$ inches.

Angle subtended by polar face, 130°.

Length of wound portion of magnet cores, 8 inches.

Winding, 120 lbs. No. 18 S.W.G. (0·048 inch diameter).

Resistance, 70 ohms.

Total turns, about 5,120.

Ampere-turns in shunt winding, 7,300.

Current through shunt coils, 1·43 amperes.

From the foregoing we get the following in C.G.S. units:—

Armature— Effective area of core, allowing for 5 per cent. occupied by paper, 297 sq. cms.

Total induction through armature core, 4,000,000.

Density, I = 13,400.

Field Magnets—Total induction through magnets, assuming leakage coefficient of 1·2, = 4,800,000.

Density, wrought iron, I = 12,400.

„ cast iron, I = 6,200.

„ air space, about I = 6,700.

Magnetising force required, by calculation:

Air space (2 l. = approx. 1 cm.) = ...	...	5,350	ampere-turns.
Armature core...	...	320	„
Magnet cores ...	...	828	„
„ yoke ...	...	642	„
Total	...	7,140	„

Mr. Sayers. This machine was not originally designed to be self-regulating when running with a backward lead, and consequently the width between the magnet cores only allows of an angular breadth embracing 2½ to 3 sections of the armature to assist the forward induction. In Fig. 10, curves 1 and 2 show the performance of the machine as a generator when run with backward and forward lead. With backward lead the position of brushes remains unchanged with all loads. The E.M.F. on open circuit is 100·5 volts at 920 revolutions per minute, at full load 95 volts. Thus, allowing 3 volts for drop in armature and brush connections, the E.M.F. generated at constant speed of 920 revolutions per minute is 2·5 volts less with full load than on open circuit. With forward lead the E.M.F. is 100 volts on open circuit at a speed of 980 revolutions per minute, and 88·8 volts at full load. Allowing 3 volts for drop, as before, the E.M.F. generated is 8·2 volts less with full load than on open circuit, and the brushes require shifting through a total angle of about 35 degrees between no load and full load.

Tables V. and VI. give the readings from which the curves 5 and 6, Fig. 11, are plotted. In the experiment the E.M.F. was kept as near as possible constant, the results with constant speed

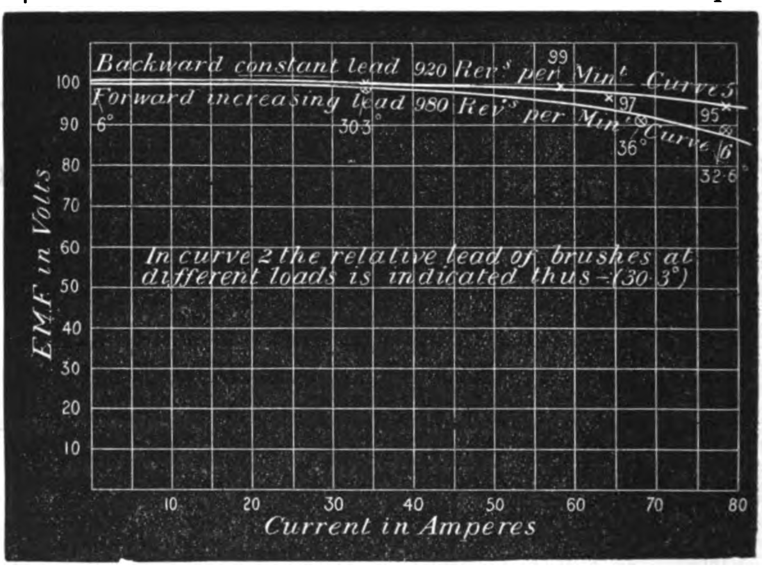


Fig. 11

being calculated therefrom. Run with a forward lead the machine displays the drop inevitable with a rather short air space. Mr. Sayers

Table I.

Speed.	E. M. F.	Current.	Position of Brushes.*	Remarks.
1,019	11·2	291	− 2°	Slight sparking.
1,000	8·35	217	+ 0·8°	Sparkless.
1,055	4·68	175	− 4°	"
1,062	2·83	78·14	− 6°	"

Table II.

All Results are Means of Three Readings.

Speed.	E.M.F.	Current.	Position of Brushes.	Remarks.
847	5·6	189	+ 1°	Sparkless.
850	8·35	289	+ 0·8°	"
850	9·9	321	− 0·4°	Slight sparking.
850	10	340	− 1·3°	" "
850	9·76	326	− 3·2°	Sparking.
850	7·1	237	− 4°	—

Table III.

Speed.	E.M.F.	Current.	Position of Brushes.	Remarks.
1,016	12·1	265	− 2°	Sparkless.
1,032	9·2	202	+ 3·2°	"

* The zero position of brushes is indicated in the diagram Fig. 9. The sign + in front of the figures indicates that the brushes were in front of the zero position, the sign − that they were behind the zero position, relatively to the direction of rotation.

Mr. Sayers

Table IV.

Means of Three Readings.

Speed.	E.M.F.	Current.	Position of Brushes.	Remarks.
991	6·1	200	+ 2·3°	Sparkless.
996	5·66	199	+ 4°	Slight sparking.
1,005	4·6	199	+ 5·8°	More sparking.
1,013	2·92	194	+ 9°	Slight sparking.
984	6·7	204	+ 0·8°	Sparkless.
991	6·78	207	− 2°	Slight sparking.
994	6·1	206	− 5°	More sparking.
1,002	5·21	204	− 7·5°	Sparking under brushes.
988	4·86	205	− 9°*	Slightly more.

Table V.

Speed.	E.M.F.	Current.	Brush Displacement relative to Position at No Load.
918	101	0	0
920	100	0	—
928	101	34	—
942	103	35	—
906	100	58	—
916	100	58	—
940	100	67	—
966	101	62	—
964	100	80	—
964	99	78	—

* If moved farther back, serious sparking occurs, so that carbon-brush gets red hot.

Mr. Sayers.

Table VI.

Speed.	E.M.F.	Current.	Brush Displacement relative to Position with Backward Lead.
978	97	0	6·6°
940	98	0	6·6°
1,002	100	35	30·3°
990	102	34	30·3°
1,120	100	72	36·0°
1,120	108	65	36·0°
1,078	98	78	32·6°
1,116	100	79	32·6°

I think I am perhaps entitled to say that all the machines I have made have been made to fill orders, which they have successfully done. So far, my experience has been that the shorter the air space the better as regards sparking and fixity of the collecting position. If solid magnet cores are used, as they must usually be on account of the expense of laminated cores, the length of the air space is limited by the width of the slots. In this connection I have found that the slots may be 1·5 times the length of air space without any appreciable heating of the poles or waste of power from eddy-current in them. If the armature is wound over with iron wire of sufficient thickness compared with width of slot, or if the conductors are in tunnels, the length of the air space can be fixed having regard to mechanical considerations only. In this connection, however, it is necessary to take into consideration the unequal pull of the keepers, or magnets, if the armature is a little out of centre; for, unless the shaft is very stiff, this may be sufficient to cause the armature to touch against the keeper when excited.

My machines, made by Messrs. Mavor & Coulson, of Glasgow, of course, run with a forward lead as motors, the forward armature turns still increasing the induction.

I have perhaps hardly laid sufficient emphasis upon the fact

Mr. Sayers. that increase in the armature current increases the strength of the flux horn, and so tends to automatically bring about the sparkless reversal of the sections without any shifting of the brushes.

I think it will be abundantly clear that, so far from having fully described what may be done by means of my device for bringing the commutation of ring and drum armatures under independent control, I have only touched on the border of a new field of design for continuous-current machines.

Professor Thompson. Professor THOMPSON: I believe that I am only expressing the sentiments of all the assembled members when I congratulate Mr. Sayers upon his paper, and upon the information which he throws out for us. I take it, from his last sentence, that there is more to follow that at the present he does not choose to tell us. However that may be, he has certainly a claim to be congratulated on having, at least so far as we can judge from the data he has given us, not only tried to do what a great many of us have tried to do, but upon having succeeded. And a man who succeeds, even though he is not the first to try, is the man to be congratulated upon his success. It happens that I once saw a very peculiar ring-wound machine—an exaggerated case, in fact, of that to which Mr. Sayers refers of an armature with wide driving horns, causing dissymmetry in the winding. It was in a dynamo built by an engineer in Paris, who wished to make the manufacture more simple in the winding of rings to get over the shuttling of the wire. He proposed to build the ring in two parts, and to wind each separately, and screw the two together by bolts. That involved mechanically two wide unwound spaces at the two sides of the ring where they were put together, where the nuts and bolts were. Consequently he designed a commutator to match, which had 20 parts on one side and 20 on the other, and a wide angular space filled up with dummy segments to match the space where there were no coils, exactly as the author of the paper now recommended. The machine sparked abominably. The constructor then took off the commutator which had its segments so spaced out, and put on

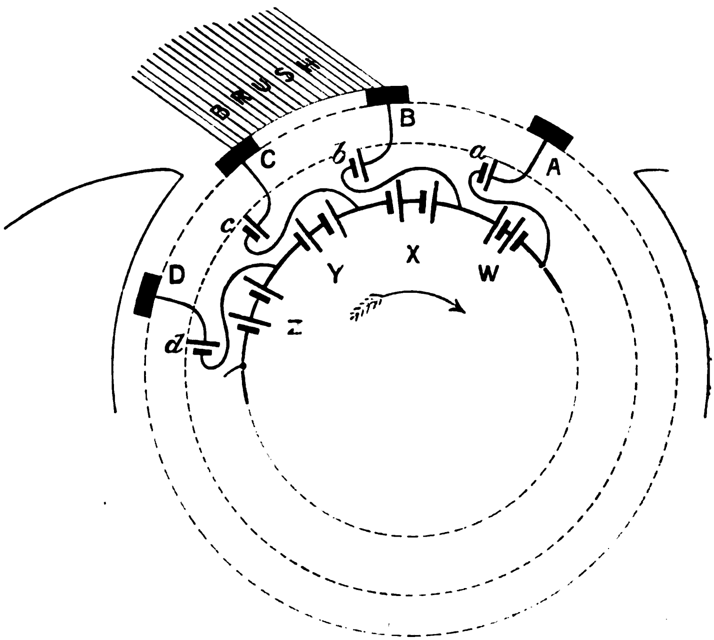
Professor Thompson.

an ordinary commutator with 40 equally spaced segments, and connected these with the unequally spaced coils. Though there was still some sparking, still there was much less than with the commutator specially designed to fit the segments of the armature. With respect to the point raised at the end of the paper about the narrow air space, of course those who took an interest in the details of machines were anxious to see armatures that shall be more mechanical than those that have gone before. So, when Mr. Swinburne first brought out those armatures with conductors carried through tunnels in the core-discs, and when Mr. C. E. L. Brown began to build them in Switzerland, it seemed as if the end of all things had been reached and there would be no more armatures with outside winding.

Somehow the end of all things had not come. The armatures with wires carried in tunnels, though they had many great advantages—one being the getting rid entirely of eddy-currents in the copper—still possessed the unfortunate property that they sparked badly; so much so that, if the speaker was rightly informed, those magnificent machines designed by Mr. Brown for aluminium production at Neuhausen, which had the conductors carried through tunnels, had to be sent back to have their core-discs turned down, and the winding put outside, as in the old-fashioned way of winding Gramme rings, before they would consent to work without sparking. Mr. Sayers had now shown us the legitimate way of returning to that mode of construction where all the copper lies in the tunnels inside the iron; and he was particularly to be congratulated on having found such a simple and mechanical mode of meeting the difficulty. Mr. Sayers had given a number of drawings, that now hung before them on the screen. Looking at the red and blue lines, illuminated by the somewhat orange gas light which falls upon them, it is a little difficult, unless one is near, to see which lines are blue and which are black. Mr. Sayers will therefore perhaps pardon me if I venture to make a little diagram upon the slate which will illustrate the winding in a way that may be intelligible to everybody irrespective of their perception of colour. Though I was not brought up entirely on

Professor Thompson.

zinc and copper, I have an affection for that way of drawing circuits in which anything that can contain an electro-motive force is indicated by the usual symbol for a battery cell. If I rightly apprehend what Mr. Sayers is doing, it is this: In the first place, he departs from the Gramme mode of connecting up the whole of the coils of the armature in series with one another, so that his windings do not entirely constitute a closed coil. Some of the coils of the armature are not in the series, but are inserted in the connectors which pass from the ring at intervals to the bars of the commutator.



Let W, X, Y, Z be four of tne sections of the winding of the ring armature, grouped together in the usual way. Let A, B, C, D be four of the bars, or segments, of the commutator. Then, instead of simply joining each bar by a connector to the junction point between two adjacent sections of the winding, the connecion was made through what Mr. Sayers called a "commutator "coil." For example, instead of the bar B being joined straight to a point on the ring winding between W and X, it was

joined to that point through the "commutator coil" *b*, occupying a position further back on the armature. In reality, the coil at *b* would be wound on the ring core between Y and X, or even between Z and Y, the connection being carried back. The consequence of this mode of winding was that at the moment when any section, say X, was short-circuited as the two bars B and C passed under the brush, there was a local circuit, C *c* X *b* B, in which there are three electro-motive forces, two of which, X and *b*, being electro-motive forces of self-induction, act in the direction of the current and tend to prevent it from dying down, whilst the other—namely, that in *c*—is in the opposite direction to that of the current, and must indeed be sufficiently great to reverse it during the time that the section is short-circuited. The coil *c* is purposely connected set back so that it shall be passing under the flux horn, and so be actively inducing a sufficiently great electro-motive force. Professor Thompson

Mr. SWINBURNE: It is not too much to say that in a few years we shall look back npon this paper as describing the greatest advance that has been made in the practical design of direct-current dynamos during the last ten years. That sounds a great deal to say, but I do not think it is too much. I think that the more we look into this method, and the more we look into the developments of it that may come—in fact, the more we refer to that part which Mr. Sayers, according to Professor Thompson, has not given us—the more we realise that there is more to follow. I will now only refer to one or two small points. With regard to the maximum load, or the maximum output, of a dynamo, Mr. Sayers states that it was given in 1890. I may mention that it was first given in 1887. I worked out the formula and published it in that year. In the days when that formula was published, and up to the present moment, the output has been limited by the various quantities to which Mr. Sayers referred. He has now introduced a new state of things altogether. The output of a dynamo is no longer limited by the cross induction. I do not know exactly what it is limited by, unless it is by the ampere-turns you can get on an armature without overheating, and perhaps the difficulty arising from Mr. Swinburne

Mr. Swinburne. hysteresis on what Mr. Sayers calls the flux pole when very high inductions are employed. In a large machine with a small air space there may be very high induction under the leading horns, and that may give rise to hysteresis when notches are employed. I do not know whether it will be serious. From what Mr. Sayers says of his experience it does not look as if it ever would be. In that case it may give us a chance of increasing the outputs of the machines enormously. I do not think that Mr. Sayers has laid enough stress in his paper (indeed, I do not think he has said anything about it, except verbally) on the importance of the smallness of the commutator connections. One is rather apt on seeing the drawing of a machine like this, or even such an ingenious diagram as that of Professor Thompson, to think that it will be complicated when put in practice; but it must be remembered that the connecting wires have only to carry heavy currents for a short time. That is an important practical matter in the design of such armatures. With regard to the unevenly spaced armature, I do not want to say anything critical when we ought to speak entirely in the spirit of congratulation on a paper like this, but I cannot help thinking that Mr. Sayers is wrong theoretically about the dummy commutator segments. We went into the matter at Messrs. Crompton's, but we did not try it, as we came to the conclusion that it would not do. The sparking would not be cured by putting in a dummy section. With reference to smooth armatures, it seems to me that the days of smooth armatures are numbered. The only reason why we have not had hole-wound armatures for a long time is the difficulty of sparking. I may mention that when I first advocated these hole-wound armatures in this country I was quite aware of the difficulty of sparking, and it was at the same time that I proposed various reversing pole-pieces for getting over the difficulty. I thought a great deal about these reversing pole-pieces, and discussed them a great deal with Mr. Crompton at the time. We never made one, as we came to the conclusion that, though they would undoubtedly work, they really were not practical—in fact, that they were not good enough to make commercially. I agree with Professor Thompson that, though

many have tried, the man who succeeds is to be congratulated. But in mentioning engineers who succeed, I am not sure that we ought not to refer to Mr. Sayers only, because I think that Mr. Housman also has succeeded in making reversing pole-pieces, which work admirably. I do not think that engineers recognise the enormous advantage of hole-winding. Not only do you get a mechanical machine and armature that you can take out and roll about on the floor without hurting it; you get rid of the Foucault currents, as Professor Thompson has already pointed out. But there is another much more important matter which is generally forgotten. When you are working with a hole-wound armature you have no mechanical forces on the armature wire. The whole mechanical and magnetic force comes on the iron, and not on the copper. That is a matter of very great importance when you are dealing with insulation. You have no longer to get your torque communicated through cotton and shellac and materials of that sort; the conductor is simply lying gently in the hole, and the whole torque comes on the iron. Mr. Swinburne.

This is a very important paper, but I will say nothing more beyond asking members to read the last sentence of the paper again, and to believe it.

Mr. R. H. HOUSMAN: In connection with this extremely interesting and instructive paper, a form of diagrammatic representation of the current during commutation is given below (Fig. 1), which I think is very useful. Mr. Housman.

A C represents the current flowing through one half of the armature, and B D the current through the other half. Supposing the commutation to take place perfectly regularly and smoothly, the current during commutation would be represented by a straight line, A B; but in practice the current-curve during commutation is not a straight line, or anything like it. The current varies, as in the two curves shown. The shape of that particular curve depends a great deal upon a gradient of the field in which the reversal of the current takes place. If the field is extremely steep—that is, if there is a rapid variation of E.M.F. from section to section—these curves will be correspondingly steep, showing the existence of the large currents, which

Mr. Housman. I believe Mr. Sayers thinks do not exist. But I think that in some cases they do exist. There is strong evidence of them,

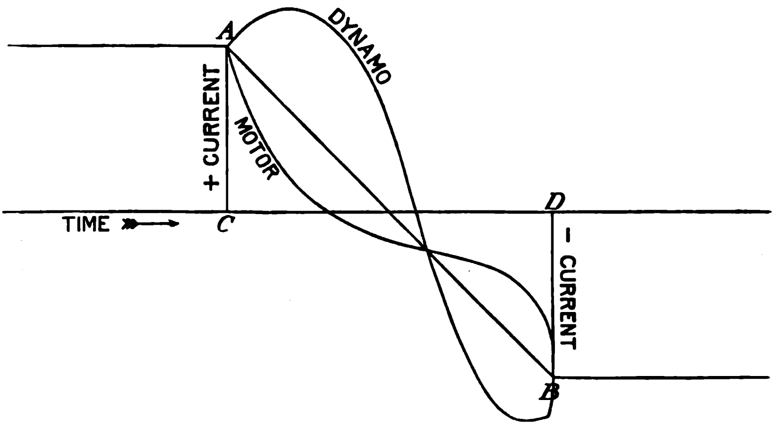


FIG. 1.—Variation of Current in a Section.

which is shown by the fall of potential at different parts of the brush surface, and by the variation of speed when the brushes are fixed. I think the variation of speed is due to the fact that the back current-turns of the armature are not simply those due to the current-turns included in twice the angle of lead; it is the current-turns included in twice the angle of lead, plus or minus the average current during commutation. You frequently have curves something like those shown in Fig. 2. All the shaded area is

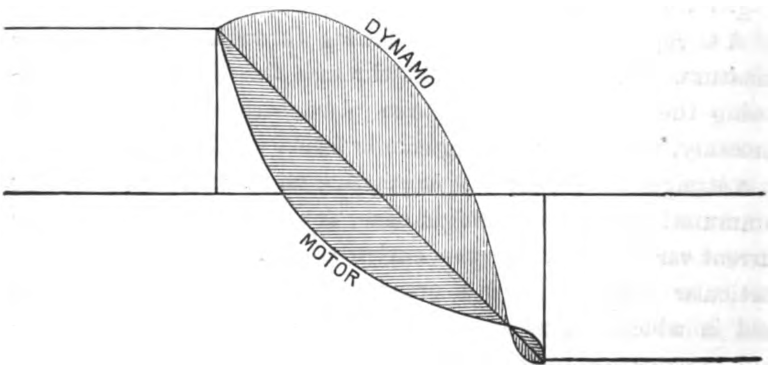


FIG. 2.—Average Current during Commutation.

employed in diminishing the field of the dynamo or the motor

in the case shown. I think it is a great deal due to this that there is a difference between dynamos and motors. Mr. Housman.

Some eighteen months ago I made a number of experiments at the Thames Ditton Works of Messrs. Willans & Robinson, by the kind permission of Mr. Willans, and to a very great extent they bore out the fact that the average current in the commutated section may be extremely large in some cases.

Then I think there is a point in connection with commutation which is too often lost sight of, namely, the extreme importance of the surface resistance between the brush and the commutator. It is evident that the segment under the front part of the brush will carry the whole current so long as there is no change in the current in the short-circuited section: there is consequently a fall of potential at this surface, unbalanced by any fall of potential at the surface of the other segment under the brush; this is equivalent to an E.M.F. in the section joining the two segments tending to hasten the reversal. If, on the other hand, the reversal proceeds uniformly at the proper rate, the current carried by either segment is proportional to the surface in contact with the brush; hence the potential differences at the two surfaces are equal, and there is no reversing E.M.F. exerted. In the case of ordinary copper brushes the surface resistance is about 1-400th of an ohm per square inch; in the case of carbon brushes it is three or four times greater, as far as I can measure. I find that with an ordinary working full load on the dynamo you may have an average fall of potential of 0·3 or 0·4 volt between the brush and the commutator. By shifting the brush forward and backwards the difference of potential between the segments under the brush may vary about one-half a volt in some favourable cases; and that half a volt represents a reversing E.M.F. which has, in a very much smaller degree, something of the same effect as Mr. Sayers's arrangement. But the same effect may be produced to almost any extent by having the connections from the armature winding to the commutator made of high resistance. It is not an uncommon thing in America for motors, where the brushes have to remain fixed for all loads, to have quite a high resistance connection between the

Mr. Housman. armature winding and the commutator. The action is much the same as previously explained in connection with the surface resistance. Then, with regard to carbon brushes, I think one of the principal benefits of them is, that if the current is crowded up into the tip it does not fuse and stick to the commutator in the way that copper brushes do if overloaded. It is often to be noticed that if a copper brush is overloaded the commutator immediately begins to get rough, which, I think, is due to the fusion of the point of the brush owing to the current being crowded up at the tip; and as soon as the commutator begins to get rough the condition of sparklessness is at an end. There is no appreciable fall of potential at the surface, and the two surfaces get into contact.

Note.—The resistance at the surface of the commutator does not act by merely reducing excessive current during commutation; it tends to produce a uniform change of current, and just before the segment leaves the tip of the brush the corrective effect is very marked. But for this, the non-sparking range of the brushes and variation of current would be very much more restricted than it is in ordinary machines. By combining a carbon and a copper brush in close contact, with the carbon in front, a reversing E.M.F. of 1·5 volts or so may be obtained, but this is accompanied by considerable heating of the commutator and brushes. High-resistance commutator connections, as ordinarily used, tend only to check the excessive currents due to a steep gradient of reversing field. If, however, each segment be subdivided into two or more parts, each of which is connected with the same point on the armature winding, a reversing E.M.F. is exerted, especially if the leading connection have a lower resistance than the others. In all these cases there is a loss by heating in the connections; but this need not exceed 1 or 2 per cent., which is less than that often caused by excessive current during commutation in ordinary machines.

Mr. Sayers, by winding his commutator connection back into the armature, very ingeniously utilises the armature field for supplying the reversing E.M.F. There seems to be a slight difficulty in the non-sparking position of the brushes being

rather limited, though this is minimised by the combination of copper and carbon in the brushes. With properly designed reversing pole-pieces, on the other hand, the non-sparking range of the brushes is very wide, and the compounding effect may be varied from about 3 per cent. up to 3 per cent. down. The machine will also excite on short-circuit, but I have not experimented on running without shunt winding on the magnets. Mr. Housman.

Mr. MORDEY: I have not fully grasped this paper yet, but I am glad that Mr. Swinburne has said what I was going to say as to its great importance. I think it comes very well indeed from him, because he has done perhaps more than anyone to lead up to the results that Mr. Sayers has attained. I think it is the most pregnant and suggestive paper on direct-current dynamos that we have had since the paper by the two Hopkinsons in 1886. Mr. Sayers has only touched on the border of a new field of design, and we shall soon see a great deal done in that direction. I think it is nearly always the case when any real advance takes place that everybody says it is a very simple thing, and that it is a great wonder it was not thought of before. No doubt the result that he has arrived at can be traced back step by step. I think that is the kind of thought that many of us must have had to-night. One must heartily condole with the writers who have dealt with the subject of dynamos in endless columns of mathematics, and especially with this branch of the subject, but who have never arrived at anything like as practical a result as this. But I think perhaps it may be possible to go a little too far in following out this direction in which Mr. Sayers has been working. I want to get information upon this point: I do not quite see the necessity for having all the excitation on the armature. In some ways it is the best place for excitation, but in others it is the worst. It is the place where you can least spare space. You cannot work with a nice low current-density there as you can on the fields; and here in this little machine that Mr. Sayers has brought before us we have the field coil giving the initial magnetisation. I should like to know from Mr. Sayers what the difference in the current-density is in the exciting coils—the armature considered as an exciting Mr. Mordey

Mr. Mordey. coil, and the field considered as an exciting coil—because I think this will show how very much more economical at least a certain portion of the excitation must be, as far as energy is concerned, if it is placed on a field magnet. It has been asked what the limit of a machine of this type is going to be. I suppose Mr. Sayers will tell us the limit is simply the heating. If the machine is self-regulating and sparkless, and there is no mechanical drag on the conductors, I think there can be no limit except the heating. I am asking for information—I cannot discuss the details of the paper, for I have not studied them well enough—but is it not a fact that high-tension and direct-current dynamos, hole-wound armatures or tunnel-wound armatures (to adopt the new word Mr. Sayers has used), are rather difficult to wind? The machines that have been wound in Switzerland and constructed in that way have all been low-tension machines; but when you have to put through a bundle of wires in order to get a high tension, and to insulate those wires from one another and from the frame, and to make connections at the ends, it seems to be a much more difficult matter, especially in small machines.

Mr. Walker. Mr. S. F. Walker: I wish to make a few remarks on the practical side, and to take upon myself the unthankful office of throwing a little cold water from the practical manufacturer's point of view. I find fault with the channels and with the small clearance, from a practical point of view. I have had some experience of the machine known in old days as the Pacinotti machine, and this, I take it, is another form of that machine come up again, but with certain modifications, possibly certain improvements. The difficulty with the Pacinotti machine was, as Mr. Mordey has mentioned, that of insulation. It is difficult to insulate wires properly when you have to place them in a channel or a tunnel: you may call it what you like. Secondly, there is the greater amount of heating. The iron core gets hot, and the wires confined in the channel are subject to a very much higher temperature in proportion to the current they have to carry, than when they are wound merely on the surface of the core. In those days I was interested in the matter and worked at

it. If you turned the core down, as I understand was done in the case of the French machine mentioned by Professor Thompson, and filled in the inner space so as to bring the core up to half the distance, you got a much better machine, and it kept much cooler. Then, with regard to clearance, I find fault strongly with the small clearance—1 millimetre, 1-25th of an inch. I was not surprised to find that he has had the armature touching many times; I should have been much surprised if he had not. We cannot work so close as 1-25th of an inch. Good fitting will do a great deal; the better fitting you can have, the nearer you may go. But you have to consider not only the fitting, but the hands the machine has to go into. You have to consider that your bearings will wear, and that the spindles will bend unless they are very stiff; and then you come to other problems; and to a certainty you will get the armature out a little; it will be rubbing on the poles from time to time, and you will do more harm than good. Then I would call attention to this: In regard to all these devices for reducing sparking and for increasing efficiency, is the game worth the candle? When you get another 1 or 2 per cent. efficiency, is it a real efficiency that you have got? Is not the real efficiency of the machine what it costs to maintain—that is, the interest on its first cost, the cost of the coal, the oil, &c., that are used, and the cost for repairs and attendance? It is an old rule in engineering that when you get beyond a certain figure every 1 per cent. costs more than it is worth. Of course that figure is advancing, and I think that all practical engineers will welcome papers of this kind, and welcome advances of this kind that are made, because, although (I do not want to discourage the author) they are rarely permanently successful, except in a few special cases, they do a great deal of benefit in advancing the standard at which the really efficient machine comes in. Mr. Walker.

The PRESIDENT: I am sure many gentlemen present would like to study this paper carefully. There is a fortnight before us, and we will therefore adjourn the discussion to Thursday, May 25th. The President.

The PRESIDENT announced that as the result of the ballot the following candidates were elected:—

Foreign Member:

Kal, E. M.

Associates:

Blenheim, William.
Brade, George Alfred.
Neale, John Edward.
Rorke, Thomas Joseph.
Wolley-Dod, Thomas Crewe.
Young, Charles Townsend.

Students:

Bennett, Arthur Edward.
Brady, Maurice Hollinshed.
Carpenter, Herbert Lant Viret.
Duncan, Edward MacGregor.
Hopton, Charles Alfred.
Sperati, Joseph.

The meeting then adjourned.

The Two Hundred and Fifty-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 25th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 11th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced.

The PRESIDENT: This is the last meeting of this session, and it is customary on these occasions for the meeting to give its consent to these last applications being balloted for forthwith. The Council have very carefully considered the claims of all concerned, and they present these candidates to you for election to-night. Is it your pleasure that they be balloted for in the usual way?

The proposal was unanimously agreed to.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Arthur Brooksbank.
Robert Charles Graham Clark.
Bertram Annandale Guiseppi.
William George Hibbins.
J. H. St. Hill Mawdsley.
Graham T. W. Olver.

Mr. Weekes and Mr. Woods were appointed scrutineers of the ballot.

The SECRETARY read the following letter from the American Institute of Electrical Engineers:—

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,
12, WEST 31ST STREET, NEW YORK,
May 15th, 1893.

Mr. F. H. WEBB, Secretary,
Victoria Mansions, 28, Victoria Street,
Westminster, London, England.

MY DEAR SIR,—I take much pleasure in informing you that the World's Fair authorities have placed at our disposal two rooms adjoining the official headquarters

in the Electricity Building at Chicago, which I am expected to occupy as the headquarters of the Institute during the Exhibition season. The rooms will be fitted up with all essential conveniences, and the members of the Institution of Electrical Engineers will be welcome there at all times. Letters may be forwarded in the care of the Institute, and I shall be pleased to be of any possible service to our visitors.

The House of the American Society of Mechanical Engineers at 12, West 31st Street, New York City, where our regular headquarters are established, will also be available to your members while in this city. Arrangements are now being perfected to make it a General Bureau of Information for visiting engineers, and a special invitation has been extended to friends of the American Institute of Electrical Engineers and the American Institute of Mining Engineers to consider the House of the Mechanical Engineers their resort for such facilities as it affords. It is conveniently located in respect to the finest hotels and theatres, and with elevated railway and cable tramway facilities to other parts of the city.

Trusting we may have the pleasure of meeting many of our European friends during the season, I beg to remain,

Yours very truly,
RALPH W. POPE,
Secretary, American Institute of Electrical Engineers.

The PRESIDENT: I presume, gentlemen, that it is your pleasure that the Secretary be instructed to acknowledge with thanks this communication from the American Institute.

The proposal was unanimously agreed to.

The PRESIDENT: I have now the pleasure to announce the decision of the Council in respect of the award of the Salomons Scholarship for 1893.

Partly in consequence of two years' dividends on the £1,000 originally presented by Sir David Salomons for the foundation of the Salomons Scholarship having been received before the first award was made, and partly in consequence of Sir David Salomons's generous additional gift of £500 to the same fund, as announced in February last, the Council find themselves in a position to award this year two scholarships of £35 each; and they have awarded one such scholarship to Mr. F. R. Lydall, a student of King's College, and one to Mr. J. T. Morris, a student of University College, London.

We will now resume the discussion which was adjourned from the last meeting on Mr. Sayers's paper. Does Mr. Sayers wish to add anything?

Mr. Sayers. Mr. W. B. SAYERS: I only wish to say, before the discussion is resumed, that, owing to the courtesy of Messrs. Mavor &

Coulson, I am able to show you an armature on my principle. It proved rather too heavy to bring into the room, but those gentlemen who have not already seen it will find it in the hall. Mr. Sayers.

Professor Ayrton.

Professor AYRTON: The paper that Mr. Sayers gave us last time interested me very much indeed, as, indeed, it must have all those who heard it. It interested me especially because in the paper he revived the old notion that Professor Perry and I put forward some years ago of using the armature field of a machine to help the main field, instead of arranging the fields to fight against one another according to the usual plan. Mr. Mordey, in the remarks that he made, raised a doubt as to whether a certain amount of power for magnetising could be more economically employed in the winding of the armature than in the windings round the field magnet; but that, I think, is not the proper way to look at the matter. It is not a question whether it is more economical to produce a field by directly exciting the armature than by directly exciting the field magnet, which was what Mr. Mordey was considering. That is not the question in point; it is whether it is more economical to produce a field by simply exciting the armature alone, or by producing a forward magnetisation, $F + A$, by means of the field magnet, and $- A$, by means of the armature, which is what is usually done. There can be apparently no doubt as to what is the answer, for it is obviously more economical to produce a field by one magnetisation than by two opposing magnetisations. In fact, a dynamo as usually made —the dynamo that is the envy of the present manufacturer—is somewhat analogous to a cart which is pulled forward by a powerful horse and pulled backwards by an energetic donkey; and matters are so arranged that the greater the load in the cart—the greater the load on the dynamo—the more vigorous becomes the retrograde donkey.

Now what was proposed years ago was simply to turn the donkey round and make him help the horse—let them both pull together. That that was possible, and that it was an end very desirable to achieve, was proved, for example, by tests that I made ten or eleven years ago in Paris, by measuring the efficiency and power of a motor with various leads: the more the lead was

Professor Ayrton.

forward, the better as regards efficiency and power the motor worked. I also—in May, 1883, I think—showed at a lecture at the London Institution that with a forward lead of a motor it was not necessary to excite the field magnet at all. A motor was shown running with the armature only having a current passing round it; the field magnet had no wire on it at all. A little later on Dr. Hopkinson gave a paper at the Royal Society on an analogous experiment, viz., that with a back lead a dynamo could be made to excite itself without any winding on the field magnet. But the orthodox makers of dynamos, perhaps not unnaturally, objected to these arrangements, because they said that, going back to the analogy, if you put your donkey side by side with the horse, he would kick, and strike sparks with his hoofs. Now how does Mr. Sayers look at the matter? He looks at it, I think, in the sensible way. He says: "Let the donkey "kick, and try to strike sparks: we will prevent him. We will not, "because sparks would be produced, or might be produced, with a "good arrangement, abandon that good arrangement, but we will "adopt what is good and neutralise what is evil; we will arrange "an armature field to help the main field, or employ the armature "field alone to produce the working field, and, having done this, "we will stop the sparking"—by what he has called the "commutator coils." I think those present last time did not grasp the essence of the charming suggestion that Mr. Sayers really meant in his paper, and I think that was due to the incomprehensibility of Mr. Sayers's own diagrams. I have ventured, therefore, to make a rough diagram (Fig. 1), which I believe I am right in saying really shows the arrangement Mr. Sayers uses, but, to make the thing simpler, I have shown the commutator coils as if they were wound on a separate armature. There is a certain coil, A, which is being short-circuited on the main armature, and there are two other coils, B, C, which are brought into play on the auxiliary armature. The arrows represent the direction of the electro-motive forces in the armature coil, A, and in the two auxiliary coils, B, C, which are now operative; e_2 is the larger of the two auxiliary electro-motive forces, because it is produced by an auxiliary coil, B, that is in a better part of the field than the other auxiliary coil, C,

Professor Ayrton.

which generates an E.M.F., e_2, helping the E.M.F., e_1, of the coil, *A*, of the main armature, which is in the act of being commutated.

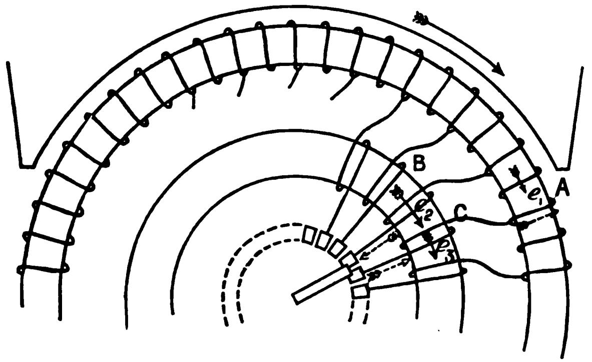


Fig. 1.

The result is to produce a current, shown by the direction of the dotted arrows, flowing in the opposite direction to the E.M.F., e_1, of the commutated coil. By Mr. Sayers's device, which for simplicity I have represented by putting the commutator coils on a separate armature, he has succeeded in commutating a coil, not where it has usually been commutated, but where it is best to commutate it.

The only defect in this arrangement is that, while it is extremely compact, and I doubt not extremely satisfactory in its operation, it has the disadvantage of utilising part of the main armature to wind on wire which is not operative in producing the main external electro-motive force. You have to have a number of auxiliary coils merely for the purpose of neutralising the spark and reversing the current. There are other plans which have been thought of for producing the same result. One plan, of course, is to use an auxiliary electro-magnet to produce the required reversing electro-motive force. One is to shape the field magnet in a particular way at the place where you want to commutate, so as to introduce an auxiliary electro-motive force. The worst of all, so to speak, is the ordinary plan of commutating in the wrong place, because at that place there happens to be an electro-motive force naturally introduced which will reverse the current in the armature. There is one plan that Professor Perry

Professor Ayrton.

and I discussed some years ago, but I do not remember at this moment whether it has ever been published. It may, however, be worth mentioning. It has the advantage that you do not alter your dynamo at all, and you are able to work your dynamo with a negative lead or to work a motor with a forward lead, so that the magnetisation due to the current round the armature helps the magnetisation due to the current passing round the field. The plan is to introduce an outside electro-motive force, and to use two brushes at each side of the dynamo. You will see by the plan that we have four brushes, two at each side. Consider the coil, A (Fig. 2), which has to be commutated. Let

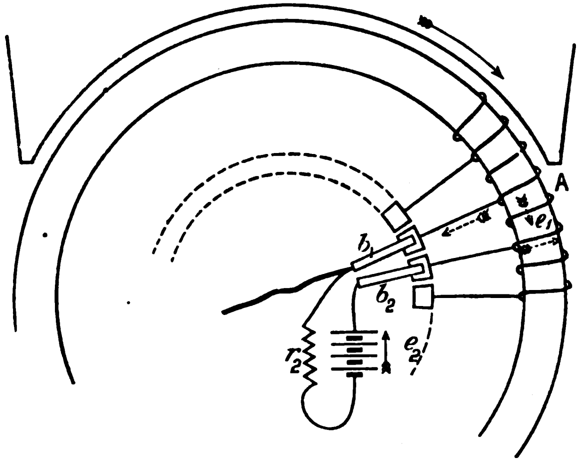


FIG. 2.

it have a resistance, r_1, and let e_1 be the E.M.F. in this coil that has to be overpowered. Between the pair of narrow brushes, b_1, b_2, at each side of the armature introduce an E.M.F., e_2, by means of accumulators, for example, and a non-inductive resistance, r_2. Then, when these brushes touch two adjacent pieces of the commutator, as in Fig. 2, the current flowing through the accumulators and the armature coil will be,

$$C_1 = \frac{e_2 - e_1}{r_1 + r_2};$$

and when the two brushes b_1, b_2 touch the same commutator piece the current will be,

$$C_2 = \frac{e_2}{r_2}.$$

For example, let e_1 be 0·5 volt, and r_1 be 0·001 ohm, and let the dynamo be producing 180 amperes—that is, let the current through each of the coils in half the armature be 90 amperes. Then, if e_2 be 5 volts, and r_2 be 0·049 ohm, we have, Professor Ayrton.

$$C_1 = \frac{5 - 0{\cdot}5}{0{\cdot}001 + 0{\cdot}049}, \text{ or 90 amperes;}$$

$$C_2 = \frac{5}{0{\cdot}049}, \text{ or 102 amperes;}$$

that is to say, the required reversed current is produced in the armature coil when commutated, and the current through the accumulators is only increased 12 per cent. when the two brushes b_1, b_2 touch the same commutator piece. The accumulators are not, therefore, damaged, and, as we have assumed an E.M.F. of as high as 0·5 volt in the coil being commutated, we have assumed the commutation to be performed considerably behind the position in which it is usually effected.

You can therefore commutate without sparking, as Mr. Sayers has done, more or less in any part of the circumference. I do not think this plan is as neat as Mr. Sayers's: it is not self-contained; it requires an outside battery; and the resistance, r_2, would require adjusting as the output of the dynamo varied. At the same time this adjustment of the resistance r_2 could be effected automatically, and so the method might be worth trying, because it might perhaps lead to a means of enabling you to commutate without sparking at the part of the field at which Mr. Sayers performs this operation, even with an ordinary dynamo which has not an armature specially wound nor pole-pieces specially constructed.

For regular work Mr. Sayers's plan would be much more economical, but for experiments on a dynamo or motor *as ordinarily constructed* this four-brush and auxiliary E.M.F. plan might prove useful.

I think Mr. Sayers is to be very much congratulated for having, as I venture to think, revived the right principle of making the machine, and for having introduced a very ingenious contrivance to enable that right principle to be utilised without the destructive sparking which has always hitherto accompanied it in practice.

Mr. Snell. Mr. A. T. Snell: I should like to add my congratulations to those that have already been given to Mr. Sayers for his neat solution of this important problem. The problem is an old one, and as long ago as nine years, when working with Mr. Swinburne, I tried a similar plan to that Professor Ayrton has just now illustrated on the board, but it was not successful.

Professor Ayrton: Did you try it properly?

Mr. Snell: I think it failed chiefly because the position of commutation did not remain constant. Mr. Sayers's device aims at stopping sparking and reducing the air gap, and, consequently, the excitation: all of us who have had to do with designing dynamos and motors know what this means. I am sorry that Mr. Sayers has hitherto only applied this principle to Pacinotti armatures, for I am under the impression that there will be considerable mechanical difficulties in their application to smooth-core armatures. In the course of my experience with this class of work I, in common with others, have tried the toothed armature, and I have had, after an extended experience, to go back to the smooth core. And I cannot help thinking that Mr. Sayers, after a round of experience, will also have to retrace his steps. There are a few difficulties which strike me in connection with the application of the commutator coils. The first is the question of heat. We know that at the present moment the limits to the output of a large dynamo are sparking and heating. My experience, at any rate, is that the output of a large armature is practically restricted by the heating. The heating in the commutator coils, I will admit, is small; but I am afraid these series exciting coils are in the wrong place. If we put them on the field magnets we may have to use more wire and expend more energy; but the heat we waste there can be readily controlled, whereas to the armature which is already overheated by the natural condition of affairs we cannot very well afford to add more. The largest armature that Mr. Sayers has built, up to the present time, seems to be one adapted to about 40 units. I roughly calculated the output on my slide rule before I came in. I estimate that it should give about 35,000 watts; so that if it will give 40 units successfully, the objections I

have just raised do not seem to have much weight. There is one application of Mr. Sayers's principle the utility of which appeals to me at once, and that is in connection with series-wound motors. The problem of designing series-wound motors for traction purposes has been complicated very largely in the past by the question of weight. With a stationary machine weight can be neglected; on a tram-car it cannot; and if we can lessen the weight of a machine by 15 or 20 per cent. by means of this device—and it seems to me quite possible to do so—a very material advance will be made in the design of motors. I may also point out that the device seems to be chiefly applicable to series-wound machines, for there there seems to be a possibility of entirely doing away with excitation, but with compound-wound machines only the series coils are obviated, the shunt winding being still necessary. From a conversation I have had with Mr. Sayers, I find with compound machines, so far as he has experimented, unless compound coils are used, the external characteristic curve is a straight line, whereas in practice we require an ascending curve—in fact, the machine has to be slightly over-compounded. So that, it appears to me, unless the number of commutator coils be increased—that is, two commutator turns instead of one (which means increased complication)—it would still be necessary to use a few series turns; and then the question is, whether by using exciting turns on both armature and field we have gained much. Mr. Sayers's invention undoubtedly suggests considerable improvement in the design of direct-current motors and dynamos, and I cannot help thinking that manufacturers are not very pleased to see it, for its general adoption must have the effect of depreciating existing stocks. However, I am not a manufacturer now, and I am very pleased to welcome it and all its possibilities. Yet it always seems to me that the ultimate end of these matters must be the banishment of the commutator, and I am looking forward to the day when dynamo and motor will work without sparking, simply because there will be neither commutator nor brushes to spark. Mr. Snell.

Professor GEORGE FORBES: I would only say, in connection with the introduction of these secondary coils for overcoming Professor Forbes.

Professor Forbes.

sparking, that I have lately seen a dynamo in which that device was introduced, but it was introduced to get over a somewhat different difficulty. It was the device of one of the most ingenious American electricians, Mr. Eickemeyer. He was experimenting in a field which has been put forward as a very hopeful one indeed for the use of the alternating current for motive power—that is, the employment of a series-wound direct-current motor with a laminated field. I do not think that most people are aware of what the chief difficulty is in using such a motor with the alternating current. Of course a series-wound motor, when you pass a current in at brush A and out at brush B, goes in a certain direction; but if you reverse the current and pass it in at brush B and out at brush A, it goes in the same direction; and therefore it seems right and natural that if you use the alternating current both impulses would be in the same direction and it would work well. The difficulty which has been experienced in the use of laminated fields, leaving out of account the difficulty arising from self-induction—which is well known, and which is largely reduced by lowering the frequency—is that there is very violent sparking at the commutator when starting. The reason for this is obvious. When the armature is at rest, and the alternating current is passing through the field, it is magnetising and de-magnetising the armature at a rapid rate, and this induction is passing through the short-circuited coil—the coil of the armature which happens to be short-circuited by the brush at the moment. There is no resistance in that coil except the resistance of the coil itself, and therefore there is a very powerful current indeed generated while the armature is stationary or nearly stationary; and that has been, as much as the self-induction, the reason why direct-current motors with laminated fields have not been more largely used with alternating current. I mention this partly because it is a fact, I believe, not generally known, and, secondly, because Mr. Eickemeyer introduced a device almost exactly the same as this—in principle identically the same—that is, to arrange the commutator in such a way that the short-circuited coil was always opposed by a counter electro-motive force generated by separate coils subjected to the magnetic field.

These are all the remarks which I have to make on this very interesting paper. The subject has been under discussion for two nights now, and I did not hear the remarks that were made last evening, and if I were to say anything more it would very likely be merely a repetition of what has been said by some other speaker, and therefore I close my remarks. Professor Forbes.

Mr. ALEXANDER SIEMENS: I must say that I was astonished at the title of the paper, because I thought that sparking at the brushes was ancient history, and that modern dynamos did not spark. This device introduces complications which, viewed from a manufacturer's standpoint, should, if possible, be avoided. The object which a manufacturer has to attain is to give a good machine which will produce a given output at the least cost. This extra winding which has to be put on the armature is certainly taking up useful space, and prevents you putting as much copper into the active winding of the armature in the very limited space as you can do without these coils. Therefore, if we are to adopt such a device, it should be clearly shown that it is cheaper to use this device than to adhere to our ordinary construction and avoid the sparks all the same. On account of what fell from some previous speakers, I wish to emphasise the fact that it is not at all difficult to make dynamos which will run under any load without any change of position of the brushes, and which will not spark at any time. If anyone wishes to see such dynamos, I am perfectly willing to show them. It is exactly the same with motors: there is no necessity for motors to spark as long as they are not absolutely overloaded, and I do not see the use of such a complication at all. It may do all that Mr. Sayers claims. It has certainly done good work by inducing Mr. Sayers to make experiments, and to show what can be done in that way; but I very much doubt its utility from the manufacturing point of view, which, of necessity, has to be a purely commercial one. On the other hand, if Mr. Sayers can convince me that a saving is effected by building machines as described by him, I am quite ready to adopt his designs. Mr. Siemens

Mr. W. A. CHAMEN: I shall perhaps be able to give a little explanation, as I have, by the courtesy of Mr. Sayers, seen the Mr. Chamen.

Mr. Chamen. machine since the last meeting. The machine described in the paper is wound to act entirely as a regulator, I believe, and is constructed to give about 13 volts when running with a current of about 400 amperes. It is driven by a separate motor, and used in series with the main dynamo, and in series with a feeder which carries a large number of lights at some considerable distance. It acts as an ordinary series dynamo would do when having the feeder current forced through it, though perhaps considerably better, and gives the necessary increase of voltage so as to make the voltage at the far end of the line exactly constant under all conditions of current. I have seen the machine at work, and it works perfectly, without any sign of sparking. It was not working a full load, however, there being only about 30 amperes going through it; but it was quite evident, from the appearance of the commutator, that it never could have sparked.

Mr. Mordey. Mr. Mordey: As one or two speakers have referred to some remarks that I made at the last meeting, I should like to be allowed to add a word to what I then said. I venture to think the real point of Mr. Sayers's paper is that he enables us to approximate the direct-current dynamo to a transformer. Excitation in a transformer requires very little energy indeed. In a transformer we can do without any air space. Air space costs money, because we must supply energy to force the magnetic flux through it. It seems to me the gist of Mr. Sayers's paper is the fact that he enables us to get a very small air space —the mere mechanical clearance that reduces the ampere-turns from thousands to hundreds. He does that, and, by the position in which he is able to collect, he uses the armature current for the purpose of supplying a large proportion of the excitation. These ampere-turns on the armature must be there anyhow. If they can be used for the excitation of the whole machine, so much the better. At present we do not use them properly; our present arrangement is a compromise. I quite agree with Mr. Siemens that good modern direct-current dynamos run sparklessly, but at what a cost! Consider an ordinary good modern two-pole or four-pole dynamo: look at the amount of copper on it; look at the many thousand ampere-turns necessary to get the magnetisa-

tion of the armature! Look at the magnetic leakage outside —at the necessarily inefficient way in which one must apply the copper on the fields—a large part of it outside, and practically useless—mere end connections! It seems to me, then, that the real point of Mr. Sayers's paper is that he shows one way by which we get rid of that great air space without losing the great advantage of sparklessness, and avoid the necessity for putting a large amount of copper on the field, and of paying a large amount of money for that copper. Mr. Mordey

Mr. CROMPTON: As a manufacturer of dynamos, I should like to corroborate what Mr. Siemens said. Without wishing to dishearten Mr. Sayers from carrying on his interesting experiments, which will be no doubt of great value to us all, I must point out that I agree with Mr. Siemens in saying that the problem which Mr. Sayers must keep before him is mainly one of manufacturing cost. Dynamo machines have long since arrived at a point of sufficient perfection when regarded from the users' point of view; all further improvements must be in the direction of reducing the material and the labour used in their manufacture, and if Mr. Sayers's improvement is really one of this nature, he will have achieved a great step in advance. It is on this point that we must ask him to throw light and tell us definitely what will be the reduction in the weight, and hence the cost, of the materials used, and what reduction, if any, can be made, in the amount paid for labour in producing a dynamo of a given output, when his system of construction is compared with the ordinary one. In this question are really included the others that have been raised by Professor Ayrton, Mr. Mordey, and others who have spoken as to the possibility of great reductions in the air space, and hence in the cost of the field winding. Experience only will show what saving Mr. Sayers's improvements will enable him to effect. He is working at a problem which has for the last six or seven years been before dynamo designers. I need only mention the names of Mr. Swinburne, Mr. Brown, of Baden, Wenstrom, of Stockholm, and Messrs. Ganz, of Budapest, as among those who have been seeking to economise the cost of winding of the field magnets by making sunk-wound armatures. All these Mr. Crompton.

Mr. Crompton. gentlemen have in turn considered they have got over the difficulties entailed by sunk winding in causing sparking and wear of commutator, but, speaking broadly, they have all of them failed; and if Mr. Sayers has now really succeeded, I for one join in heartily congratulating him.

Mr. Swinburne. Mr. SWINBURNE: The President has given me leave to add one remark to what I have said before. It seems to be taken for granted that this form of commutator connection can only be used with sunk windings. I am not at all sure about that. If a smooth armature is used, there is, of course, a large air space; if there is a large air space, it takes a less electro-motive force to cause a reversal in the section, because a certain number of ampere-turns in that section cannot produce anything like the same local field; and, at first blush, it seems to me that we ought to be able to use it almost as well for smooth as for hole-wound armatures. Of course the difficulty is that the reversing field is really the fringe, and what is wanted is to get one reversing connection in the field, and one well out of it. There is considerable difficulty in doing this with a smooth armature, as the commutator connections ordinarily come close together, and the fringe is larger, so that the change of field is less abrupt; so that it would be scarcely possible to get one connection well out of the field while the other was well in. But I should like to ask Mr. Sayers if it is not possible, by lessening the number of commutator sections, to get over that difficulty. There would be fewer commutator connections of more turns each. I am speaking without thinking it out carefully. Professor Forbes has referred to the difficulty of making a series alternating motor of an ordinary series-wound direct-current machine with laminated fields. As Professor Forbes has pointed out, there are at least two difficulties. I think there are really a great many more; but there are two main difficulties, which are so great that they practically swamp the others. The first is the difficulty due to the self-induction of the machines—to use again a term which I object to rather strongly, but which is allowable in this case, I think. That, of course, can be overcome to a certain extent by condensers; but there is a serious objection to their use, because

people do not want condensers in their houses with motors. The other objection is one which, as Professor Forbes says, has not been very fully realised—namely, the difficulty of the sparking at the commutator. As Mr. Mordey has pointed out, the armature may be regarded as a secondary. In this case we have a transformer; one circuit is on the fields, and there is a secondary on the armature; and that armature will develop very large currents when the brushes short-circuit a section—very much larger currents than the electro-motive force generates—because the induced electro-motive force is as great as the armature electro-motive force would be in a section if the machine were running at one revolution per period. I think that might be got over by winding the armature double. If the armature is wound double, as is sometimes done for very large currents, there are two complete circuits round the armature. If your brushes are only the breadth of one segment they never short-circuit any section, they simply pass from one armature circuit to the other. In that way I think it would be possible to get over the difficulty altogether. At first it sounds as if I were proposing to only half-utilise the armature, because it might be said that if the current is always passing in one circuit or the other, instead of passing through both in parallel, there would be waste of power; but if the brush is almost as wide as one segment, that is not so, as the two circuits are generally in parallel. They are only out of parallel just for an instant before changing from one section to the other. The result is, almost all the advantages of the ordinary commutator are secured, without the disadvantage of sparking. I have never made a motor of that sort, because the various other objections seem to me so great that I have never even had the courage to tackle the problem. Mr Swinburne.

Mr. A. P. TROTTER: Might I emphasise the remarks Mr. Mordey has made by asking Mr. Sayers to incorporate in his reply an estimate or an outline design of dynamo with his winding, and a dynamo as constructed according to modern practice? Mr. Trotter.

Mr. W. B. SAYERS: I hardly know in what terms to express my appreciation of the exceedingly kind way in which this Mr. Sayers.

Mr. Sayers. paper has been received. I do not know whether I feel competent to reply to the remarks that have been made. I am sure that I cannot reply at all fully to the various criticisms that have been offered.

Dr. Thompson kindly suggested a diagram which was very highly appreciated. My own diagrams do not seem to have been readily understood. Though they are difficult to understand, they represent everything that happens, and that is not true of any other diagrams that have been suggested. Dr. Thompson's diagram simply indicated the electro-motive force. In order to properly comprehend what is to happen with the machine, it is necessary to indicate in the diagram electro-motive force, resultant electro-motive force, current flowing in opposition to electro-motive force, ampere-turns, demagnetising coils, and so on. I have tried to develop Dr. Thompson's diagram a little, thus:

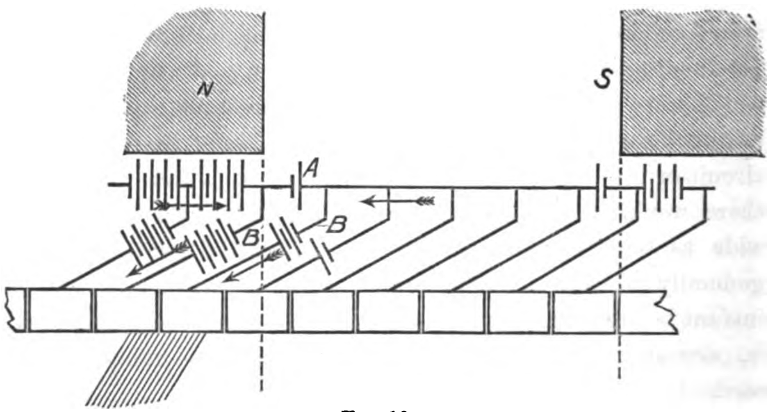


Fig. 12.

N, Flux horn. A, armature coil being commutated, represented as having 1 volt being generated in it owing to its proximity to flux horn. B, commutator coil, with 2 volts being generated in it. B′, commutator coil well under flux horn, having 4 volts generated in it. The resultant electro-motive force in the circuit completed by the brush, and composed of commutator coils B and B′ and armature section A, is 1 volt in the direction necessary to bring about the reversal of the current in armature

section A. The current in the armature sections situate between the pole-tips will be of such direction as to magnetise the armature instead of to demagnetise it, as would be the case if the brush was set forward instead of backward as regards the direction of motion. But I do not see how in such a diagram we can represent the magnetising effect of the current. This form of diagram no doubt makes it clearer how the commutation can be effected. Another point that Dr. Thompson touched upon was the use of dummy segments in the commutator where driving pins are used, but he instanced a machine in which the armature was in two halves and bolted together. As I understand the machine was made in the early days, I think it is quite possible that at the joints where the two halves were bolted together there may have been considerable eddy-currents, and that would entirely upset the state of affairs. Mr. Swinburne also referred to the dummy segment, and I think he confirmed Dr. Thompson's view that it was useless. I have not tried it; I do not believe in armatures with irregular spaced winding, but I offered this to some people who seem still to find irregular spacing necessary. Mr. Swinburne thought that we might be troubled in the future with hysteresis in the flux horn. I have never seen the slightest sign of heat in the flux horn or any other part of the field-magnet circuit, provided the air space is not less than two-thirds of the width of a slot. But I should perhaps qualify that by saying that it is also necessary that the aggregate cross sectional area of the teeth under one pole at any time should be sufficient to carry the induction. I think that is a point which has been overlooked by some gentlemen, especially those who complain of heating of the teeth in slotted armatures. I have never found the teeth in armatures of my own design heat, and I think the reason is that I have always observed the necessity for making the sum of the cross areas of the teeth equal to the cross area of the core. We have a certain amount of induction to pass through the armature, and if we pass it through teeth, it is evident that we must be as careful to make the aggregate cross section of the teeth under the pole-piece at any time large enough to carry the induction, Mr. Sayers.

Mr. Sayers. as we are to make the cross section of the core large enough. I am much obliged to Mr. Swinburne for pointing out that in a slotted armature there is no force exerted on the wire. That is a point which had not occurred to me, but it certainly seems a very strong one in favour of the construction in which wires are either wound through holes or in slots.

Mr. Mordey suggested that there was no practical use in putting the whole excitation on the armature. I quite agree with him, more especially with regard to small machines. It is well known that the excitation required on a small machine is very much greater in proportion than on a large one. The excitation in any given type is proportional to the linear dimensions only of the machine, whereas the output is approximately as the cube. It therefore follows, I think, that the reactions upon which I depended in my device will be greater in large armatures than in small ones. I think it is far more useful in large machines—that is my experience. Large machines behave quite as well, or better than small ones. Mr. Mordey also suggested a difficulty in winding through holes. I may say that there is no difficulty, or very little, if you set about it in the right way. He also thought that there would be a difficulty of insulation. I admit at once that I have never made any very high tension machines with wires wound into slots, but I cannot possibly see any difficulty about getting good insulation. The insulation of the armature that I have downstairs, when tested, was over 2 megohms. With regard to insulation of the conductors from the core, it is quite easy to use thin mica in insulating such conductors. Of course I know it may be said that 2 megohms means nothing: you want a certain thickness of insulating material if you have a high electro-motive force, but I cannot see any difficulty. I may say here that although it is true that with machines designed as at present space is most valuable, it is nothing like so valuable when you adopt a slotted or tunnel armature, and for this reason: with a smooth-core armature you are limited to a given air space; if you increase it, you have to put more ampere-turns on the magnet, and increase the weight and cost of machine; but if you use a

slotted or tunnel armature there is nothing to prevent you increasing the diameter of the armature and making deep narrow slots. You have not to increase anything else in proportion. By increasing the diameter of the armature you can make the slots as deep as you like, and put almost as much copper in as you like. If you put another half-inch on to the diameter of the discs it does not increase the cost of the machine to any appreciable extent. Nothing else is increased in proportion. In smooth-core machines, if you attempt to increase the diameter of the armature, you at once make a bigger machine, but it is not so in the case of slotted or of tunnel armature machines. With regard to Professor Ayrton's arrangement, I have not thought it out properly, but I fancy that his proposal to put a cell in between the brushes cannot work at all, and for this reason—that you cannot alter a current which is flowing in the coil; you cannot perform the commutation without the identical current which you have to commutate flowing through the circuit. If you work it out you will find that is so. Consequently, the resistance which you are obliged to put in to avoid short-circuiting the cells by the commutator segments is absolutely fatal; you cannot get the current through it. I see Professor Ayrton shakes his head. I may be wrong, but that is the impression I have. Mr. Sayers

Mr. Snell also thought that I was wrong in going back to toothed armatures. Perhaps what I have said about the possibility of increasing the diameter of such armatures without making substantially a bigger machine answers him to some extent. Mr. Snell also spoke about heating. I think he said that he had found trouble from heating of the teeth. I may repeat that I have found no trouble from heating of the teeth, provided there are a sufficient number of them, or that the aggregate area is approximately equal to the cross section of the core. Many of the early toothed armatures were made something like this. (Fig. 13).

That is perfectly absurd. If you have teeth, you want them to carry the same induction as you want to get through the mass of the iron core. It is impossible that such as those shown in Fig. 13 should do it even if magnetised up to the very extremity of

Mr. Sayers. saturation. Mr. Snell mentioned the fact that I have not up to the present time made a machine to compound up. That is quite true; but it is not in the least difficult to do so. I think it will be admitted that if you can make a machine to self-excite without any coils on the magnets, it is easy to make a machine to compound up. If I were to put shunt coils on the limbs of that machine illustrated in Fig. 9, which excites without any winding at all on the magnets, it is obvious that it would compound up far more than is ever required in practice.

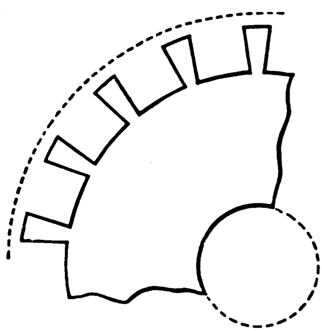


FIG. 13.

Professor Forbes spoke of an alternating-current motor in which a somewhat similar device to mine had been used. I may say that I have also been on that track. About three years ago I designed a machine, which was never made, on those very lines. I had an arrangement for introducing an opposing electro-motive force in the coil which is short-circuited under the brush, because I saw the induction went through that section; and in addition to that, I also put a winding round the armature to counteract the effect of the armature coils. A short air space machine with laminated magnets of course acts precisely like a transformer: you hardly get any current through it; but if you put a winding round the armature to demagnetise it you improve matters, at least. I think Professor Ryan has been experimenting with a continuous-current machine with demagnetising coils around the armature. The armature coils all tend to magnetise the ring and pole-pieces as indicated by the dotted lines (Fig. 14), and produce an induction which opposes the alternating current. If, now, you wind a coil round the armature in the opposite direction, you counteract that and still get your driving force.

Professor FORBES: May I just mention, as a curious coincidence, that Mr. Eickemeyer's motor has exactly the same arrangement?

A MEMBER: May I add that Mr. Stanley's motor has the same arrangement also?

Mr. Sayers

Mr. SAYERS: You will find it in the provisional specification of mine.

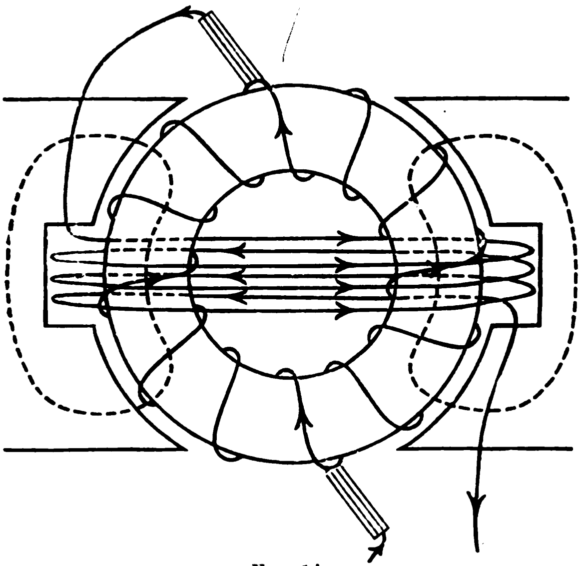


FIG. 14.

Professor FORBES: The coincidence was that Mr. Eickemeyer has the same device as Mr. Sayers had introduced for the alternating motor, both in respect of perfecting the sparking and of getting that demagnetising effect counteracted.

Mr. SAYERS: I should say that my brother, Mr. J. Sayers, was associated with me in that; I did not do it entirely alone.

Professor FORBES: Perhaps Mr. Eickemeyer had seen your patent?

Mr. SAYERS: No, it was not published. I am afraid I cannot do justice to Mr. Housman's remarks. There is a great deal of interest in them. One point that he brought out was that the reversal of the current in the section which is being commutated was not anything like a straight line. Well, I may say that I was quite aware that it was not a straight line, but I thought that I would be complicating my paper far too much if I attempted to go into such details. Mr. Housman, however, still adheres to the idea that a considerable current is generated in the short-circuited coil when passing the brush. I cannot see how that

Mr. Sayers. is possible, because if the machine is running with the brushes in the proper position—I mean the sparkless position, or the position of least sparking—when the coil becomes short-circuited there is an electro-motive force being induced in the opposite direction to the current which is flowing; consequently, instead of this current rising, as Mr. Housman represents in his diagram, it immediately begins to fall. It may not fall in a direct line to reversed value, but it cannot increase, and it is carried through zero necessarily; and if the brush breaks connection with the commutator segment at the very instant when the current in the coil is of the right value in the opposite direction, there cannot be any sparking. The current is zero at that instant, and the electro-motive force is of course zero as well. Mr. Housman cites as evidence of the supposed heavy current in the short-circuited section the difference in speed between a generator and a motor. It is a curious fact that a very simple explanation of this difference has not been published. At any rate I have not seen it, and therefore I will mention it. If you have a shunt generator to give 100 volts, then you have 100 volts on the shunt coils; you have a definite excitation. While in the armature there will be $100 + C R a =$ say, 103, in the case of the motor the shunt coils will also have 100 volts, but the armature will have $100 - C R a =$ say, 97. The matter stands thus:—

GENERATOR.

Shunt.	Armature.
100 volts.	$100 + C R a =$ say, 103 volts.

MOTOR.

Shunt.	Armature.
100 volts.	$100 - C R a =$ say, 97 volts.

Take the motor which you are supplying with 100 volts. You have 100 volts on the shunt, and, so far as that is concerned, exactly the same as a generator. But you have only a *total* of 100 volts on the armature too, and, consequently, you have only got $100 - C R a$, which will equal, say, 97; consequently, you you have got at once a difference of speed with the same machine having the same E.M.F. at its terminals as a generator and as a

Mr. Sayers.

motor of 6 per cent. When you consider that probably you will have, if your armature has a loss of 3 per cent., another 1½ per cent. or so in the brush leads and surface resistance at the brushes, you will probably get a difference of 9 or 10 per cent. in the speed of the generator and the motor with the same volts on the terminals. Then Mr. Housman also gives some useful information about the resistance between the brush and the commutator in a machine, and he also says that this tends to produce the reversal of the armature section. No doubt it does; but I think it comes too late, and I think if we look into it we shall find that if we rely upon this we have got, not a sparkless, but a sparking dynamo. Mr. Housman gives the resistance of a square inch of surface at 1-400th of an ohm. That means that if the brush which is leaving the commutator section is bearing on a strip one-eighth of an inch wide, and the bearing surface is 4 inches long, then we shall have half a square inch of surface, and the resistance will be 0·005 ohm. I think that is right. Then, a moment after, the brush will only be bearing on a strip 1-16th of an inch wide, and we have the resistance of 0·01 ohm. Now, if we have 100 amperes flowing, we shall have a difference of potential of $100 \times 0{\cdot}01$, $= 1$ volt, to produce the reversal required. But we must consider that we have got in that 1-16th of an inch 100×1 equal 100 watts. We have 100 watts being generated at that point at the tip of the brush, and I do not think it is anything to be wondered at if that brush gets hot, and, as Mr. Housman says, "fuses, and "sticks to the commutator." I think the foregoing may contain the explanation of the difference which all of us have noticed between the wearing of the two brushes. If this kind of thing does actually happen, at the positive brush we shall have these 100 watts always being dissipated in the tip of the brush, whereas in the negative brush we shall have 100 watts distributed over the whole of the sections of the commutator; consequently, we shall find one brush go very much more quickly than the other.*

* This sentence inadvertently contains an assumption which is not warranted by experience, so far as I know—*i.e.*, that heat generated at a contact surface is developed on the positive electrode. I had the idea of an arc in my mind at the time. Still, as arcing does take place at the sparking brushes, the explanation holds on the whole.

Mr. Sayers. I am sure Mr. Crompton will remember the days when we used to have these difficulties to fight against. Then Mr. Housman also refers to the use of high-resistance connections between the commutator segments and the armature winding, and he says that these act in a very similar way. I think it is quite evident that they cannot produce the reversal of the section. When one coil comes into connection with the brush without any current flowing in it, no doubt there is an electro-motive force to set up a current in it; but the moment the current in the two connections are equal, there is no further tendency to alter it, due to the resistance in the connection. Consequently the commutator segment will go out of connection with the current still flowing, and the sparking will occur. I did not quite understand from Mr. Housman whether he had actually made a machine to compound up. He said something about compounding up 3 per cent. or down 3 per cent. by means of reversing pole-pieces, but he did not actually say that he had done it. I do not know whether he meant to say so or not. While on the subject of reversing pole-pieces, I think I may remind you that Mr. Swinburne pointed out some time ago the difficulty, if not the almost impossibility, of designing them. If you bring them from the opposite horn you get fearful leakage, and if you bring them from the same horn you have induction in one direction, due to the magnetising coils of the field; and in order to get the reversing pole-pieces to operate, you have to reverse the induction passing through them. Now to do that you have to have a greater exciting force in the opposite direction than you have in your field coils. If you do it with a shunt you have to use a lot of wire or waste a lot of power; but you have not space to do it anyhow. Of course it would be far nearer practical, if it were practical at all, to do it with very short air space, because then you have got a smaller number of ampere-turns to get on.

With regard to Mr. Siemens's remarks, I quite expected to hear that kind of thing said. I think the best reply I can make is this—that I will undertake, if Mr. Siemens will send me a design of a field magnet, or if he has field magnets made of any

machine, to make an armature which would enable him to cut out one-half the length of the iron field-magnet limbs, and use shunt wire one-half of the sectional area, and so halve the weight of shunt wire, and also the watts expended in the shunt winding, at the very least. Mr. Sayers.

Mr. SIEMENS: But I am speaking of total cost. If your armature costs four times the money, and you save only half that amount by making the field magnet shorter, then your machine is worse. If you can convince me that I can make the whole machine cheaper, I will make your machines. It is simply a question of cost. It is a practical question, not a scientific one.

Mr. SAYERS: I can only say that we have made some dozens of machines on this principle, and we have not found the armatures expensive to make.

Mr. SIEMENS: That is not the question. The question is whether we do not make ours cheaper.

Mr. SAYERS: Certainly, I think that with experience at any rate you would be able to make them quite as cheaply. There is a slight increase in the amount of material on the armature, provided the output remains the same with the same depth of core. I am not able to do what some gentlemen have asked me, viz., to say distinctly what the saving would be. I think if they will take a license and begin experimenting themselves, that would be the best way to arrive at it. I think Mr. Mordey hit the nail on the head when he pointed out that what I aim at is reducing the air space and enabling us to do away with the necessity for a large air space. It was to control the commutation so as not to be obliged to make a machine with a certain air space and certain ampere-turns simply on account of the sparking. We can now by means of these coils, so far as sparking is concerned, do anything we like, and that is why I have said that it is not so much what I have done, as what may be done, in designing machines on these lines. Mr. Swinburne said something further about machines with sunk windings only. I may say that I myself have not worked out the question of using commutator coils upon a

Mr. Sayers. smooth-core armature. My object was to get away from smooth-core armatures, and to get away from copper wire wound with shellacced cotton on the outside. Mr. Swinburne also suggested multiple winding for an alternating-current motor. I do not know; I have not been able to think out during the discussion whether that would work according to my idea or not, but I daresay he is aware that multiple-wound machines of that kind were made by Weston some years ago. Then Mr. Trotter asked me to supply comparative designs. I do not know whether I can undertake to do that.

I am afraid that I have not done anything like justice to what has been said in the discussion, but I will not occupy your time any longer this evening.

The President. The PRESIDENT: It is now my pleasure to propose a vote of thanks to Mr. Sayers for his extremely interesting and able paper, which has led to very considerable discussion. There are three points in the paper, dealing with sparks, with compounding, and with governing. The question of governing has occupied a very small space indeed in the discussion. I had hoped that Mr. Sayers's brother, who has been occupied in applying that system at Bradford, and is now about to introduce the same mode at Derby, would have been here to explain to us what he proposed to do. There are very few engineers who have had charge of continuous-current systems who do not know that when the central station is at some distance from supply, as the load increases and diminishes the voltage rises and falls in a very uncomfortable way. By applying one of these governors designed by Mr. Sayers on his lines they are able at the Bradford Hotel belonging to the Midland Railway Company, and they will be able by the system now being applied to the offices of the company at Derby, practically to maintain the voltage at the supply point constant, however much the load may vary. As regards sparking, which has been dealt with so thoroughly in the discussion, and by Mr. Sayers in his paper, we must all remember that sparking is due to the existence of currents in the wrong place. It is said that a man who enables us to make two blades of grass grow where one blade grew before benefits his race; in like manner, a man benefits

The President.

the electrical profession when he is able to transfer currents in the wrong place doing harm to currents in the right place doing good. This appears to me to be one of the merits of Mr. Sayers's paper—that he has shown us how we can cure a defect, how we can save a waste of energy by utilising something elsewhere that is doing no good in another place. In fact, he utilises what is otherwise a disturbing element. It is true, as Mr. Siemens mentioned, that the modern dynamo is practically sparkless; but it must be made sparkless—you must watch it—you have to adjust your brushes. It is sparkless within certain limits, but beyond those limits you must adjust your brushes to maintain a sparkless condition; and the advantage that Mr. Sayers has pointed out to us is that by his system this sparklessness can be produced automatically. And I consider, in all electrical appliances, that whenever we can make them useful automatically we have done a very great good. It is quite refreshing in this room to hear something novel about the dynamo. We have had paper after paper dealing with theory, with the principles of the dynamo, but here we have something introducing a practical novelty. It shows that, notwithstanding all the remarks that have been made, sometimes by myself, against the dynamo, it is not yet played out. When we find such strange coincidences brought to our notice as have been done to-night by Professor Forbes and Mr. Swinburne, that the plan of Eickemeyer and of Stanley has been anticipated by Sayers, we cannot help coming to the conclusion that, however uninteresting papers may appear, they generally succeed in making themselves interesting by drawing out facts in the discussion such as we have had this evening. I sincerely hope that Mr. Siemens will jump at Mr. Sayers's offer and let him have the skeleton of a dynamo in order that he may prove how it can be made more useful at a cheaper price. For, after all, it is a question of £ s. d., and there is no use in bringing new facts of this kind before our meetings and discussing them unless the result is to be economical and useful. We know that it is going to be useful; let us hope that, by his offer being taken up, Mr. Siemens will be the means of convincing this Institution that it will also be

The President. economical. I am sure that you will carry the vote of thanks to Mr. Sayers with acclamation.

The motion was carried unanimously.

The PRESIDENT: This is the last meeting of the session. I have some hopes of meeting a sufficient number of the members of the Institution at Chicago to enable me to say that we shall hold a meeting of the Institution there. The American Institute has very handsomely invited you to make use of their buildings. They will have a meeting to receive us at Chicago, and I hope that there will be a sufficient number of members to justify our holding a meeting there, so as to be able to return thanks on the spot for the hospitalities and kindnesses that I know are being prepared for us.

I have to announce that the following candidates have been duly elected:—

Foreign Member:

Bannai Toraji, M.E.

Members:

Major Chesney, R.E.
William Chew.
Prof. James Alfred Ewing, F.R.S.
William Edward Louis Gaine.
Legh Sylvester Powell.

Associates:

Frederick Samuel Carter.
Arthur Ernest Chandler.
Richard Clay.
Thomas Crichton Fulton.
George Ireland.
Edward Price.
J. Torr Todman.
Lieut. Archibald John Saltren Willett, R.A.
Arthur Frederick Williamson.

Students:

Hugh Samuel Davidson.
Alfred Freeman.
Arthur E. Mayes.
Frank Gibson Travers.
Roderick Sidney Travers.

THE PREVENTION AND CONTROL OF SPARKING, ETC.

By W. B. SAYERS.

APPENDIX.

In the course of the discussion upon my paper dealing with the control of sparking in short air space dynamos, Mr. A. P. Trotter and several other gentlemen suggested that I should give some definite information about the relative cost and efficiency of a machine on my principle, compared with an ordinary smooth-core machine. In complying with these requests it seemed to me that I could not do better than compare a machine of my design with the Edison-Hopkinson machine, which was described in the celebrated paper on "Dynamo-electric Machinery," by Drs. J. and E. Hopkinson, read before the Royal Society in 1886.

The particulars of the Edison-Hopkinson machine given herein are partly taken from Dr. S. P. Thompson's "Dynamo-electric "Machinery," and partly from specimens of the machine, several of which I have had experience of from time to time. There is no sparking whatever at the brushes of this machine when working at full load, and I believe the limit of output is fixed by the heating and by the mechanical strength of the armature. I have, therefore, in my design kept the total magnetic flux through the armature, and also the magnetic density, the same as in the Edison-Hopkinson machine.

The total flux through armature core when the machine is running without load is about 10,530,000 C.G.S. units.

The E.M.F. is generated by 20 coils in series, which cut the field of force four times per revolution. We have, therefore, if we take the speed at 750 revolutions per minute,

$$E = \frac{10{,}530{,}000 \times (4 \times 20) \times 750}{60 \times 10^8} = 105 \text{ volts.}$$

In my design I make the number of coils on the armature 38, and the E.M.F. is generated by 19 coils in series and two "commutator coils," or "reverser bars," as I prefer to call them in a machine of this size, because they only pass once from end to end of the armature, without going round the ends. The two

reverser bars add an E.M.F. equal to one main armature coil, because, although the E.M.F. induced in one of them is only half that induced in an armature turn, they carry the whole current, and thus add their half-turn-E.M.F. to both halves of armature. Thus we have,

$$E = \frac{10{,}530{,}000 \times \{4 \times (19 + 0{\cdot}5 \times 2)\} \times 750}{60 \times 10^8} = 105 \text{ volts.}$$

The diameter of the new armature over all is $11\frac{1}{4}$ inches; at the bottom of the tunnels, $9\frac{1}{4}$ inches; and of the hole through the centre, 3 inches. The length of the core is 20 inches. The area of the iron in the core of the armature is thus, allowing 5 per cent. for paper,

$$20 \times (9{\cdot}25 - 3) \times 6{\cdot}45 \times \frac{95}{100} = 768 \text{ sq. cms.*}$$

The induction density in the armature core will be,

$$\frac{10{,}530{,}000}{768} = 13{,}700.$$

The air space I make 0·25 centimetre, or about 0·1 inch. The tunnels are shown full size in Fig. 15; they are 0·125 inch below the surface, 0·875 inch deep, and 0·437 inch wide. The crown of the tunnel is slotted through to lessen the self-induction of the conductors.

The main winding would be of rectangular conductors 0·3 inch × 0·25 inch, and the "commutator coil," or reverser bar, 0·3 inch × 0·15 inch. These conductors and the tunnels which contain them are shown in section, full size, in Fig. 15. The main conductors, C, may advantageously be made up of eight wires of rectangular section, covered together so as to form a stranded conductor. The object of the stranding is to facilitate bending. The current-density at full load of 320 amperes will be 2,150 amperes per square inch, against 2,460 in the Edison-Hopkinson machine.

---

* The area of iron of core of armature of E.-H. machine is given in Dr. Thompson's book as 810 sq. cms.; but apparently this has been reduced, as the cores of two armatures which I have had the opportunity of measuring have only had a section of about 770 sq. cms. of iron, excluding the three discs of $\frac{3}{8}$ inch thickness which are in the Edison-Hopkinson armatures.

The construction of such an armature presents no difficulty whatever. The holes to form the tunnels are punched in the

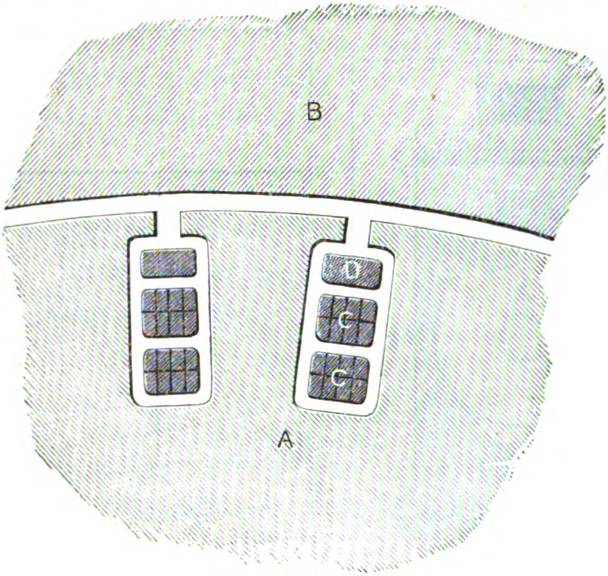
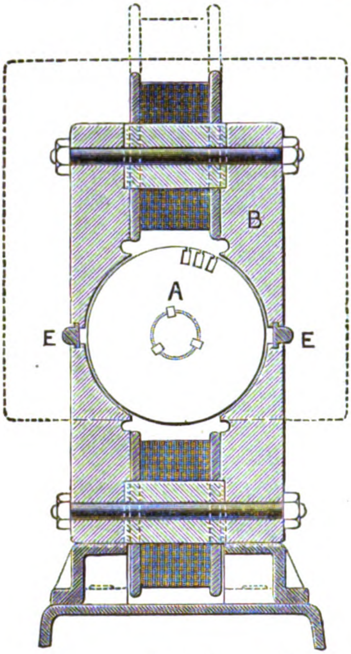


Fig. 15 —Full size.

discs before putting together, and are made a trifle larger than the required size of tunnel to allow of slight inaccuracies in the punching. Lengths of main conductor, each sufficient to form one coil, may then be cut off and formed into **U**-shaped loops. The two loops—section through one leg of each of which is shown at C, Fig. 15—which go into the same tunnel can be taped together, and the reverser bars—section of one of which is shown at D, Fig. 15—with them, and then slid in from the commutator end. When all the **U**-shaped loops are in position, their free ends may be bent and connected up in the same manner as that adopted in the Edison-Hopkinson armatures, so as to form the main winding; but instead of soldering the ends of the coils into the commutator lugs at the time of connecting up, as is done in the Edison-Hopkinson armature, the ends of the reverser bars can be soldered to the points of connection between the coils at the same time as they are connected up. The reverser bars run back through the tunnels to the commutator. Fig. 16 represents in section at right angles to

shaft the new design. Fig. 17 represents the end view of iron only of Edison-Hopkinson machine, to same scale.



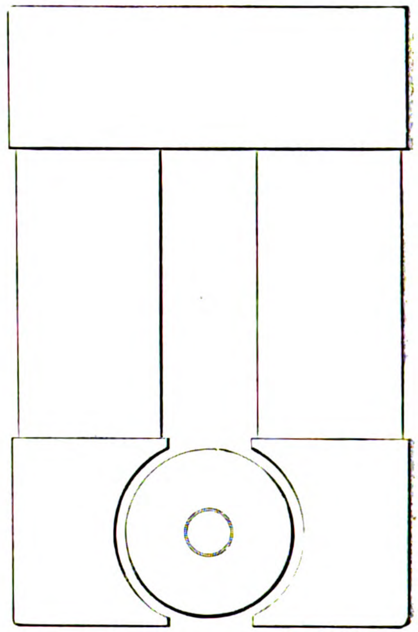


Fig. 16. Scale: 1 inch = 1 foot. Fig. 17.

The dotted line in Fig. 16 indicates a single-limb magnet. A great additional saving of weight, however, is obtained by making a double magnet, as shown in full; and the value of the iron saved more than covers that of the additional wire required. The top and bottom halves of the magnets are separated by four brass distance pieces of the section shown at E. These may be, say, 3 inches long, and placed one at each corner of the magnet limbs: gaps will thus be left for ventilation. Below are given the comparative electrical data, weights, and values of material in the two designs.

Electrical Data.

Armature.

	Edison-Hopkinson.	Sayers.
Cross section of conductor ...	0·065 sq. in.	0·075 sq. in.
Current-density ...	2,460 amps. per sq. in.	2,150 amps. per sq. in.
Resistance at 100° Fah. ...	0·008 ohm	0·0067 ohm
" of two reverser bars	—	0·001 "
Total resistance between brushes	0·008 ohm	0·0077 "

Field Magnets.

Resistance of Magnet Coils.	Edison-Hopkinson.	Sayers.
Say 15,000 feet No. 13 wire ..	16·93 ohms	—
If single magnet, 14,000 feet No. 19 wire	—	82 ohms
If double magnet, 14,000 feet No. 17 wire	—	43 "
Current	6 amperes	—
If single magnet	—	1·28 amperes
If double magnet	—	2·56 "
Ampere-turns in shunt coils ..	19,600	3,600

WEIGHTS AND VALUES.

	Edison-Hopkinson.					Sayers.				
	Weight.	Rate.	Value.			Weight	Rate.	Value.		
			£	s	d.			£	s.	d.
Armature discs	3·7 cwts.	32/–	5	18	0	4·7 cwts.	32/–	7	10	0
Armature conductor	60 lbs.	9d.	2	5	0	67 lbs.	1/–	3	7	0
Reverser bars	—	—		—		11·5 lbs.	9d.	0	8	8
Magnet cores	31·7 cwts.	12/–	19	0	0	12·2 cwts.	12/–	7	6	0
Magnet wire..	410 lbs.	9d.	15	6	0	141 lbs.	10d.	5	17	6
			42	9	0			24	9	2
Total weights of parts enumerated above ...	39·5 cwts.					18·8 cwts.				

There will be a further saving of weight of material in the bed-plate on account of the lighter magnets; and if required compound-wound, the whole cost of material and labour for this will be saved, as the new design will "compound" without any series coils.

As regards the cost of workmanship, so much depends on the details of the design, and the means employed for performing the various operations, that I think it would hardly serve any useful purpose to give my estimate for these.

In addition to the saving in weight and cost of materials, I must claim for the new design great superiority from a mechanical point of view. Each disc of the armature is positively driven by the three keys upon which it is supported; the conductors are positively driven—whether or no there is any force exerted upon them; and the armature presents a smooth iron surface which is not liable to damage from careless or unskilful treatment. The space left for insulating material in Fig. 15 is ample for 100 volts, as I have proved by considerable experience: the normal insulation resistance to the iron in such an armature would be 2 or

3 megohms. If a much higher voltage were required, the tunnels would be made deeper, so as to provide space for additional thickness of insulation, the diameter of the armature being increased to allow of the deeper tunnels.

Henry A. Mavor [*communicated*]: I regret very much that absence on the Continent prevented my attendance on the occasions when Mr. W. B. Sayers's paper was before the Institution, and that the other members of my firm were also unable to attend. I have read the appendix to Mr. Sayers's paper, and his statements as to the economy in weight and cost by the use of the reverser bars with short air space are fully borne out by our experience in the manufacture of these machines here. We were much gratified, and wish to congratulate Mr. Sayers on the interesting discussion called forth by his paper.

ORIGINAL COMMUNICATIONS.

NOTE ON A DETAIL IN PARALLEL WORKING OF ALTERNATORS.

By W. M. Mordey, Member.

In a recent paper, "On Testing and Working Alternators," I ventured to incorporate a brief account of the principles on which I and my colleagues have acted for some years when dealing in practice with the parallel working of alternators.*

In the course of the discussion on the paper a criticism was made on a certain point, and was met only very briefly. As the matter is of interest, for reasons both of practice and of principle, it may prevent misunderstanding if I allude to it a little more fully, especially as Mr. Swinburne—my critic on the detail in question—has explained his views to me at length, and insists, on theoretical grounds, that my account of the supposed facts must be wrong.

Theories may best be tested by applying them to definite and clearly ascertained actions.

The point is this: I stated in effect that two similar alternators similarly excited, driven by separate engines, could be run in parallel, and that, by sufficiently reducing the supply of steam to the engine driving either alternator, the current from that machine could be reduced to zero, or practically zero, without alteration of the excitation.

I explained that in taking an alternator out of parallel the steam should be reduced till this condition was reached, and that the armature should then be switched off; and that in putting an alternator into parallel with another the engine should have only enough steam to drive the machine at the synchronous speed; that when switched in, the alternator will "merely keep in step, "doing no work on the external circuit, and receiving no power

* *Journal, ante*, p. 131.

"from that circuit. This will be shown by its ammeter, which "will show that no current, or only a very small current, passes "between the machines." And that "the individual regulation "of current (that is, of load) of each alternator is entirely by "control of the steam supplied to each engine, and the regulation "of the E.M.F. as a whole is by the control of the excitation "simultaneously affecting all the alternators." This was criticised by Mr. Swinburne, who said that I was mistaken,—that in order to get the current down to zero, or practically zero, the excitation must be lowered till the armature gave the station pressure, otherwise the no-load current must be large enough to reduce the armature pressure, by armature reaction, to equal the station pressure.

Before switching alternators out of parallel I had been in the habit of reducing the current to "practically zero" (that is, to 3 or 4 amperes in a 50-ampere alternator—the ordinary ammeters not usually reading lower than this) without altering the excitation. Therefore I had some difficulty in believing it to be impossible.

The question, apart from all theories, is simply this: Is it possible or practicable, by reducing the steam, without reducing the excitation, to reach the condition that "the ammeter shows "that no current, or only a very small current, passes between "the machines"? This question was put to several engineers engaged in the actual running of alternators, and the following replies were received:—

Mr. Shepherd, works manager to the Bournemouth Company, writes: "I find no difficulty whatever in reducing current "practically to zero by means of governor and throttle valve. "I do not alter field excitation of the machine before switching "off, as it is unnecessary."

Mr. Johnson, manager and engineer of the Sheffield Electric Light Company, writes: "We can, and do, reduce the current to "practically zero, when switching off, by reducing the steam "supply. We do not alter the field excitation of the machine "before switching it off."

Mr. Clirehugh, engineer to the central station established by

the British Insulated Wire Company at Prescot, says: "We "succeed in reducing the current practically to zero by means "of the throttle valve without reducing the excitation. A "slight flicker is, however, observable on the lamps, which is "avoided if we reduce the excitation."

Mr. G. C. Sillar and Mr. A. J. Lawson, two of my colleagues, state, with reference to five 2,000-volt 100-kilowatt alternators installed by them at Bangkok: "The alternators were excited "in parallel from a common exciter. When about to switch off, "we reduced steam at the stop-valve until the current from the "machine was 4 or 5 amperes; probably we could have got "the current lower, but we never tried. The excitation was not "altered until after switching off. We ran various numbers of "alternators in parallel, up to five, according to the load."

In dealing with this matter in a broad and general way it is not necessary to refer to such matters as the small difference of excitation necessitated by variations in resistance between cold and hot machines. I have referred to that elsewhere. It is easily provided for by a small resistance in each alternator field, but even that is a refinement, and not a necessity, and does not affect the question at issue, as we frequently work without it.

It must be understood that, as my experience has been principally with my own alternators, the foregoing remarks are not to be taken as applicable to machines of other design; they may or may not be.

THE TRANSMISSION AND DISTRIBUTION OF ENERGY BY MEANS OF ELECTRICITY AT GENOA.

By LOUIS GOICHOT, Foreign Member.

The transmission and distribution of energy by means of electricity at Genoa is most important of those existing, and deserves special notice on account of the various features, which differ radically from all that has been hitherto done in this direction.

The Gorzente, a rapid stream running down to the Po from the top of the mountain opposite Genoa, was considered as nearly useless for hydraulic purposes on account of the enormous variations in the flow of water; but about fifteen years ago M. Bruno endeavoured to collect the water into two large artificial lakes containing together 4,600,000 cubic metres. The lakes were created by means of two dams thrown across the valley. Each of these dams is 37·5 metres high, and 50 metres broad at the base. In order to carry the water collected in the lakes to Genoa, M. Bruno built a tunnel more than 2 kilometres long, which opens at the top of the Polcevera Valley.

The aqueduct, 22 kilometres long, is built up of cast-iron pipes 60 centimetres in diameter. The source is 240 metres above the sea level. The aqueduct delivers 460 litres of water per second under 130 to 140 metres useful head. Three hundred small hydraulic motors were soon at work and the whole amount of water utilised.

As the tunnel is at 604 metres, a fall of 364 metres was available, but left unemployed. In 1888 the idea struck some people to utilise this fall by means of electricity. It was divided into three partial falls—104, 110, and 150 metres respectively—to work turbines and dynamos; the current generated was to be distributed in Genoa and along the Polcevera Valley.

The division into several falls was only made in order to avoid using turbines under heads exceeding 350 metres. Very little had been previously done in the use of such turbines, and little or

nothing was known of the manner of regulating their speed under variable loads.

The power actually transmitted at full load is about 1,000 H.P. By increasing the size of the lakes, over 1,500 H.P. may be made available.

Without going again very far in drawing comparisons which have already been frequently made, we may say a few words as to the reasons which have led to the adoption of one system of distribution in preference to another. That involves the question of continuous *versus* alternate currents.

In the case of Genoa, we have only to deal with motors. There are three ways of supplying the current to these motors, not to say anything of the multiphase system, which had not then been brought to the experimental stage, and which can as well be considered as included in the ordinary alternate-current system.

If using alternate currents, we could have, close to each motor, a transformer of appropriate size reducing the high-tension primary current into a harmless low-tension current utilised by the motor. There was nothing to prohibit absolutely the use of such a system, but, if we consider carefully the conditions, we shall see that it was not at all appropriate. The distance from the waterfalls to the town is considerable, the horse-power required is large, and consequently a very high pressure was necessary in order to have small conductors. This high pressure could only be obtained in two ways—viz., by having units producing it directly, as we do not know yet how to couple alternators in series, or by using step-up transformers. The first method leads to very expensive machines, on account of the high insulation required, and the second one to a loss of energy, due to the double transformation. Moreover, we employ powerful motors in the distribution, and it would have been necessary to provide these motors with a starting appliance. Lastly, the efficiency of alternate-current motors is not very high, and falls rapidly as the size diminishes.

Continuous currents could be used either with the parallel system of distribution or with the series system. The parallel system was, however, out of the question, as it would not be

prudent either to design or to use motors taking eight or ten thousand volts at the terminals.

On the other hand, for a long period, the series system of distribution was considered by most electricians as practically impossible when involving the use of large motors, although the enormous theoretical advantages it presents were acknowledged.

The series system is employed on a very large scale in America for arc lighting, and even for distributing power, but the distribution of power is limited to very small motors taking a few H.P. The current being generally 10 to 15 amperes, large motors would require much too high E.M.F., the pressure required per H.P. being nearly 100 volts at 10 amperes.

The motors working on a series distribution react one upon another, and have a tendency to set up in the current periodical variations interfering with the good working of the installation; but, on the other hand, the advantages we gain by using this system render it well worth trying to get rid of that inconvenience.

The series system permits the use of very high tension on the line, although having units of moderate voltage, say 1,000 or 1,200 volts. Only one conductor is necessary instead of the two required with the other systems, the result being to facilitate the extension of the circuit, to reduce the difficulty of insulation, and to require only light and cheap poles. In case of transmission and distribution of motive power only, there is no transformation whatever, and consequently a saving in the efficiency.

If dynamos and conductors are carefully insulated from the ground, there is little or no danger of accident happening to persons, as the circuit cannot be completed by the body. The conditions are the same if at the points where both conductors are fixed on the same poles, the distance between these conductors is large enough to prohibit simultaneous contact.

With the series system there is practically no limit to the extension of the distributing circuit, even when using moderate-sized cables; practically the limit is fixed by the maximum pressure we intend to use. This voltage is also dependent upon the number of dynamos working simultaneously, since, as

previously mentioned, practical considerations have led to the adoption of 1,000 to 1,200 volts per unit.

The essential point in the system is to maintain a current of constant strength in a single conductor passing through all the motors successively. The generators have then to maintain this current constant, whatever be the length of the conductor and the load on it. On the other hand, the voltage is essentially varying. When at certain moments of the day nearly all the motors are at rest, the generators have to produce only the pressure corresponding to the drop in the conductor; whilst at other moments, when all, or nearly all, the motors are working, the necessary pressure is obtained only by connecting in series five or six units of 1,000 volts each. The variations at the generating stations are consequently from one to ten, or more, and often very sudden. It frequently happens that changes of three or four thousand volts take place in a few seconds. These sudden variations of tension have been considered for a long time as an insurmountable difficulty, as they render the regulation of the current-strength very troublesome indeed, especially when, as is the case at Genoa, the current is relatively large.

The problem has been solved in different ways for each station, and these several methods I propose to describe successively. The three stations have been named from the three most renowned Italian electricians—viz., Galvani, Volta, and Pacinotti.

Generating Stations.

Galvani Station.

The first station, the Galvani, may be considered merely as a trial installation, enabling us to get a very fair glimpse of the advantages and the inconveniences of the proposed system when used on a comparatively large scale. The greater part of the energy available at that station—600 H.P. out of 750—is directly utilised by means of telo-dynamic cables in works placed in the immediate neighbourhood. The 150 remaining H.P. are given by a turbine built by Rieter, of Winterthur. It is a Girard turbine with partial injection mounted on a horizontal axle. Its maximum speed is 475 revolutions per minute. It drives directly, by means

of elastic and insulating couplings of the well-known Raffard pattern, two Thury dynamos. All the moving parts of the turbine and the dynamos are made as light as possible, in order to reduce their inertia to the utmost extent. The regulation is effected by hand by means of driving gear acting upon the valve motion. According to the load on the line, the valve is more or less opened, and consequently the speed of the turbine and of the dynamo is more or less increased in order to obtain the E.M.F. necessary to maintain the current constant. This regulating device would not do with several units working simultaneously, but it is quite sufficient for the case with which we are dealing.

The dynamos are six-pole series machines, self-exciting, with wrought-iron field-magnet cores; the armature, of the drum type, bears the Thury multipolar winding. These dynamos have a very small mass of iron in the armature core, and only one layer of conductors on the periphery. The armature reaction is nearly negligible, so that the brushes may be maintained on the neutral line. The metallic brushes are, in these machines, replaced by carbon blocks, held by means of springs against the commutator. The selection of carbon blocks appropriate to any particular type of machine requires very careful consideration, as the successful working of the dynamo depends for a great part on this selection. The hardness of the carbon must vary with the pressure it is intended for.

By means of rings dipping in a large oil tank a very good lubrication of the bearings is effected, which can be considered as quite automatic.

At full load each one of the dynamos generates about 47 amperes with a maximum pressure of 1,100 volts, but the normal figure for each unit is only 1,000 volts. This is obtained at 475 revolutions per minute. At no load the speed falls to 20 revolutions, which is just sufficient to give the 47 amperes. The extreme limits of speed to maintain the current constant on the circuit are, then, 20 and 475 revolutions, according to the load, the speed increasing with the load.

The different losses in these dynamos are about as follows:—

Excitation	1,100 watts.
Armature losses...	1,100 "
Hysteresis and Foucault currents	2,500 "

The switch-boards of the different stations being nearly identical, I will describe one in detail to represent them all. Each group of one turbine and two dynamos has its special switch-board with the following apparatus:—

A voltmeter to read up to 2,500 volts.

An ammeter for 50 amperes.

These two measuring instruments are common to both dynamos, as well as a short-circuiting device by means of which the group can be switched on and off the common circuit, and which is also used for starting the group.

Each dynamo has its own automatic switch, which short-circuits the dynamo when the voltage rises a certain amount above the maximum allowed. This apparatus consists of a solenoid of such a resistance that at the maximum pressure the current passing through it is not sufficient to attract a soft iron armature placed close by; but if the pressure rises beyond that point, the current increases in the solenoid, and its action is soon sufficient to cause the armature to be attracted. This attraction sets free a spring which by its action causes the short-circuiting of the dynamo.

Another appliance we cannot dispense with in the case of several groups working simultaneously on the same circuit is a kind of switch brought into action by a change in the direction of rotation. If, for any reason, one group in circuit is no longer able to produce the normal current, the current of the other machines, acting upon it, drives the dynamo as a motor, and possibly as a motor without any load; this would happen principally in the event of all the india-rubber rings of one of the couplings having been broken simultaneously: the result is that it can race indefinitely until the machine breaks down by centrifugal stresses. In order to avoid this accident, which might lead to very dangerous results, one property of series-wound machines was made use of. This property is, that they run in one direction as generators and in

the reverse direction as motors, the flow of the current in the field-magnet coils being always in the same direction. A special switch is geared on the dynamo shaft in such a way that as soon as the direction of rotation changes the corresponding dynamo is short-circuited.

Each switch-board is provided with two plugs, the removal of which cuts off all communication between the common circuit and the machines.

Every conductor entering or leaving a building has its lightning discharger. Storms being frequent in the mountains, these dischargers require to be thoroughly efficient. They consist in a combination of an ordinary lightning discharger with several impedance coils and a condenser.

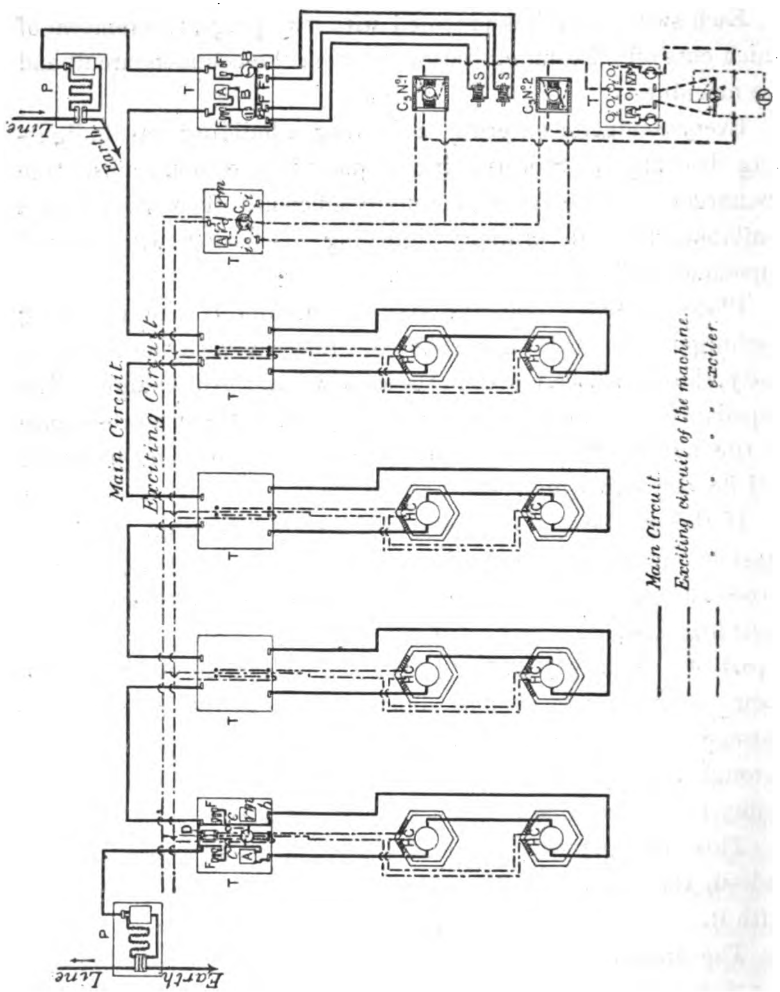
Placed between the line and the ground is the lightning discharger, then the impedance coils in series with the line, and, lastly, the condenser, placed in the same way as the discharger. The impedance coils are so arranged as to oppose little or no resistance to the normal flow of current. The working of this apparatus will be very easily understood.

If the overhead conductor is struck by lightning, the high-tension current arrives at the impedance coils, and a part of it passes through them and charges the condenser and the machines, their high insulation permitting their acting as a condenser. Then, a part of the current having passed through the impedance coils, their self-induction at once opposes such a resistance to the passage of the current that it is compelled to pass to earth through the lightning discharger without being able to cause injury to the machines.

This arrangement has proved very efficient—so much so, indeed, that all the motors on the system have been provided with it.

The iron armature core of the dynamos is supported on a gun-metal spider which insulates it magnetically from the shaft. It was thought useful to secure as good an insulation as possible between any part of the dynamos and the ground. To ensure this, thoroughly insulating materials have been laid between the spider arms and the iron core. These materials are principally

mica and shellacced cloth. In order to avoid sparking between the conductors of the armature and the pole-pieces, the whole surface of the armature is covered with layers of paper and shellacced cloth.



PLAN OF VOLTA STATION.

The bed-plate of the dynamos is supported above the ground by very strong porcelain insulators having a circular channel filled with rosin oil. The insulation of the machines from the

ground may thus be considered as perfect. On the other hand, the insulation between the dynamos and the turbines is also excellent, the only communication between them being the india-rubber rings of the elastic coupling.

Volta Station.

M_4 Excitation of the exciters.
T Switch-boards.
R Regulating resistance for exciter.
A Ammeters.
B Switches.
C 5 Exciters.
VM Voltmeters.
C Commutator.
c Commutator for voltmeter.
i Fusible cut-out for „
HC Dynamos.
F Plugs for insulating one group from the common circuit.
I Short-circuiting device on the dynamo switch-boards.
D Automatic switch acting on excess of voltage.
b Terminals.
S Solenoids acting upon the slide valve of the governor of the turbine.
P Lightning dischargers.

This station, utilising the second partial fall above the Galvani, disposes of about 700 H.P. At the present moment four groups have been erected, each composed of one 140-H.P. turbine driving two dynamos. The Girard turbines with partial injection have been made by Faesch and Piccard, of Geneva; they revolve on a horizontal axle, and are provided with a very efficient governor, in order to maintain a speed as constant as possible throughout all the variations of load.

It may not be considered out of place to fully describe this governor, as its adoption in several central stations where compound-wound dynamos are used has resulted in the maintenance

of a constant voltage, thus reducing the supervision to a minimum, and permitting of the employment of only ordinary workmen for this work.

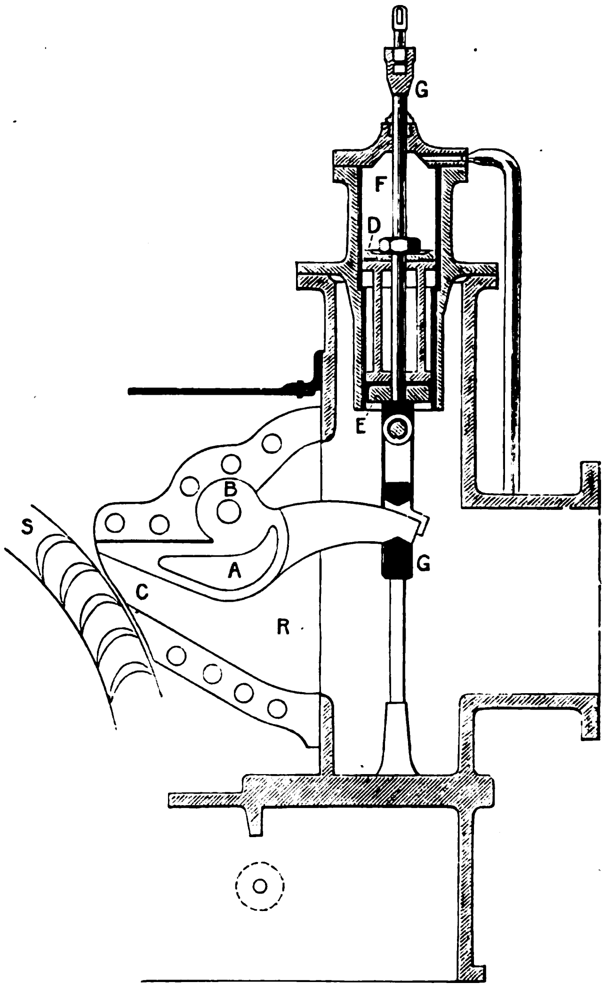


FIG. 1.

The working of this governor is as follows:—A gun-metal piece, A (Fig. 1), placed inside the turbine valve motion, R, revolves around an axle, B, and enables the opening through which the water passes on its way to the turbine wheel to be varied. This piece is acted upon by the rod, G, of two unequal pistons, D and E.

The lower one, E, which is the smaller, constantly receives underneath the pressure of the motive water. The top piston, D, is acted on by the water contained in cylinder F. But cylinder F can communicate either with the motive water or with the exhaust. In the former case, when the motive water under pressure can go into the cylinder F, the rod G is driven down and port C is open, because piston D is larger than piston E. In the second case, when cylinder F communicates with the exhaust, the rod G is driven up, as, then, only piston E receives underneath the pressure of the motive water. The appliance which causes cylinder F to communicate either with the motive water or with the exhaust is a small circular equilibrium slide valve, I (Fig. 2), which opens or closes ports conveniently drilled in the gun-metal cylinder K, the action taking place as in a steam engine.

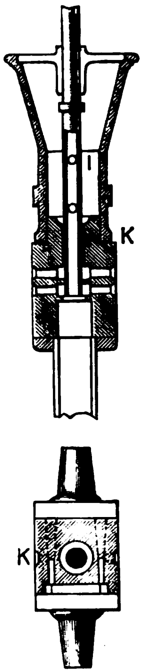


FIG. 2.

These ports are disposed in such a way that when the slide valve I comes down, cylinder F communicates with the water under pressure; when, to the contrary, it goes up, this cylinder communicates with the exhaust. Lastly, when the slide valve is in the middle of its stroke, the cylinder F is closed, and the rod at rest.

In the mechanism we are occupied with, the slide valve I is not only acted upon by the governor, H (Fig. 3), but also by a lever, L M N, set in action simultaneously by the governor at the point M and by the rod O L, which receives its motion from the rod G, by means of the lever Q O P oscillating around the fixed point P.

When the governor is lifted up a certain amount, the lever L M N oscillates first around point L and raises the slide valve I, thus establishing a communication between cylinder F and the exhaust. The immediate result is that rod G and point Q are lifted up, and that lever L M N, by the action of rod O L, oscillates now around the point M lifted up, and the slide valve

comes down until, arrived at the middle of its stroke, the movement is stopped.

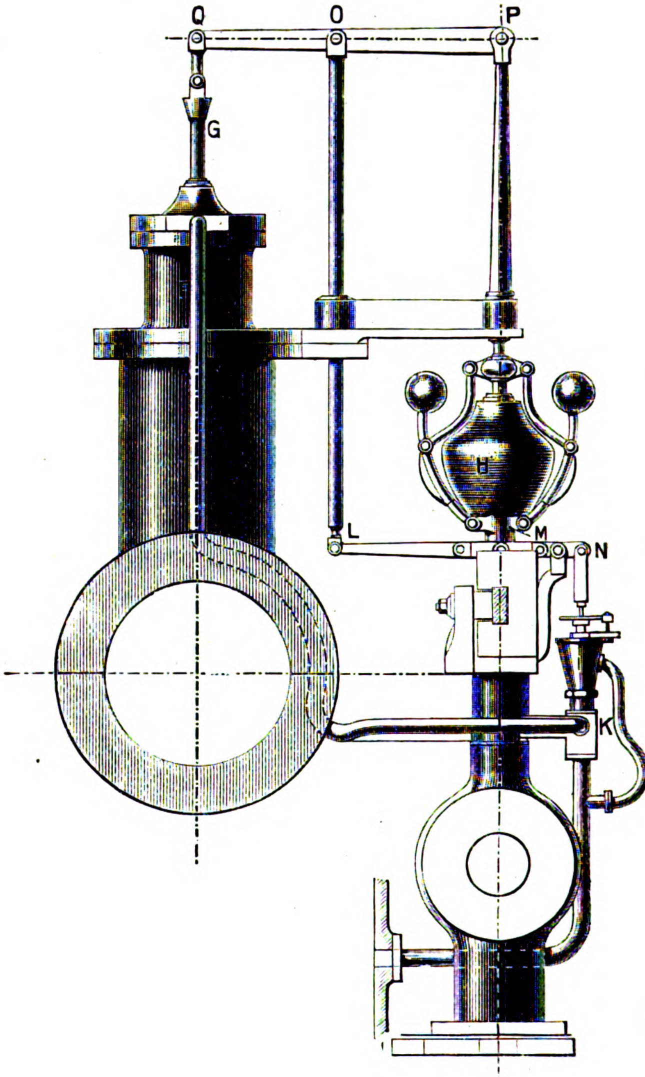


Fig. 3.

Thus, after each displacement of point M, the point N comes back to its starting place: it results from that, as well as from the manner in which the levers are connected together, that

any determined movement of point M occasions a movement of point A or of rod G, this being the magnified reproduction of the movement of point M—that is to say, of the governor.

That is to say, the rod follows in amplifying all the movements of the governor, as if point M were fixed and the governor powerful enough to drive directly the rod G. But, whilst this rod follows all the movements of the governor under the action of a very high water pressure capable of overcoming all resistances due to friction, the governor itself has nothing more to do than to move the small slide valve I, which presents no resistance whatever to the action of the governor.

In fact, it is a direct-acting governor sensitive enough to prevent variations of speed of more than one or two per cent. when half the load of the turbine is suddenly taken off.

The dynamos are of the same type as those used at the Galvani station, but instead of having the moving parts lightened, these are made as heavy as possible, in order to increase the inertia. This is done on account of the means of regulation adopted to maintain a constant current. In this station, this is arrived at by maintaining the speed constant and varying the excitation. The dynamos are separately excited by a small machine driven directly by a 15-H.P. turbine.

This system of regulation was adopted on the special demand of the Societa dell Acquedotto, and notwithstanding the objections of the constructors.

As all the dynamos of the station are connected together by being excited in parallel, the danger of accidents is much increased, as it might happen that, by some injury to the insulation, 8,000 volts would pass through each machine instead of the 1,000 volts normally allowed. In order to obviate this danger, a high extra insulation has been provided for the machines at this station; it consists of thick layers of mica placed between the electro-magnets and the bed-plate.

A very serious inconvenience inherent to separate excitation arises from sparking at the commutator. This difficulty was overcome by using a very strong magnetic field and coupling the exciting winding in a suitable way. Furthermore, a special

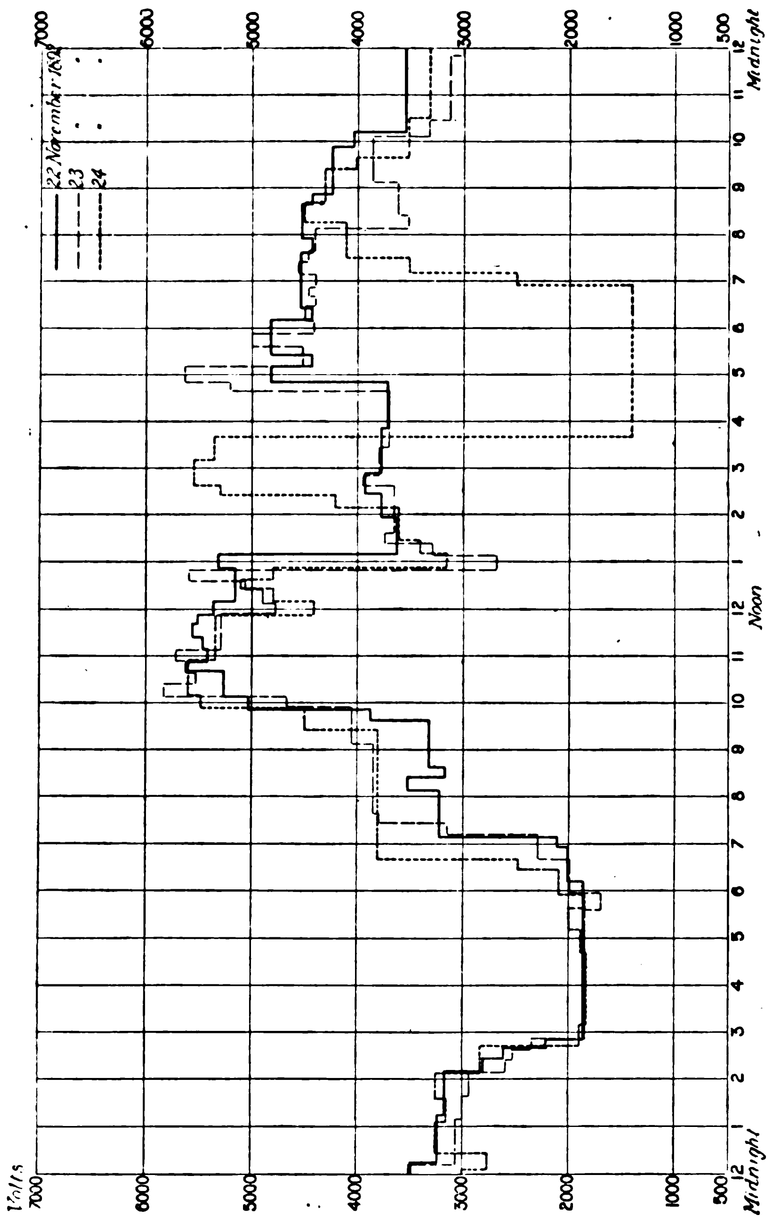

Volts
7000
6000
5000
4000
3000
2000
1000
500
12 Midnight
1
2
3
4
5
6
7
8
9
10
11
12 Noon
1
2
3
4
5
6
7
8
9
10
11
12 Midnight
22 November 1892
23
24

FIG. 4.

kind of carbon blocks of relatively high resistance have been provided to collect the current on the commutator, and they have proved very useful in suppressing sparking. In fact, the result arrived at is so satisfactory that when short-circuited the machines show no sparking at the commutator, and that without any shifting of the brushes. As a rule, everyone knows that the part of a dynamo which requires the most careful supervision, and which is the most expensive to keep in repair, is the commutator. In these machines, after 16 months of continuous running, there is not the slightest trace of wear, and the carbon blocks are nearly as good as at starting.

Fig. 4 shows the variations of pressure at the Volta station during three consecutive days; they were not recorded by

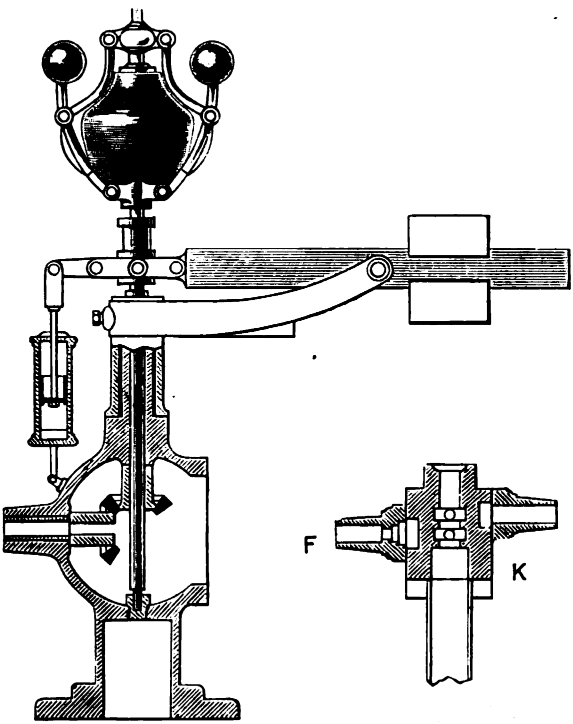
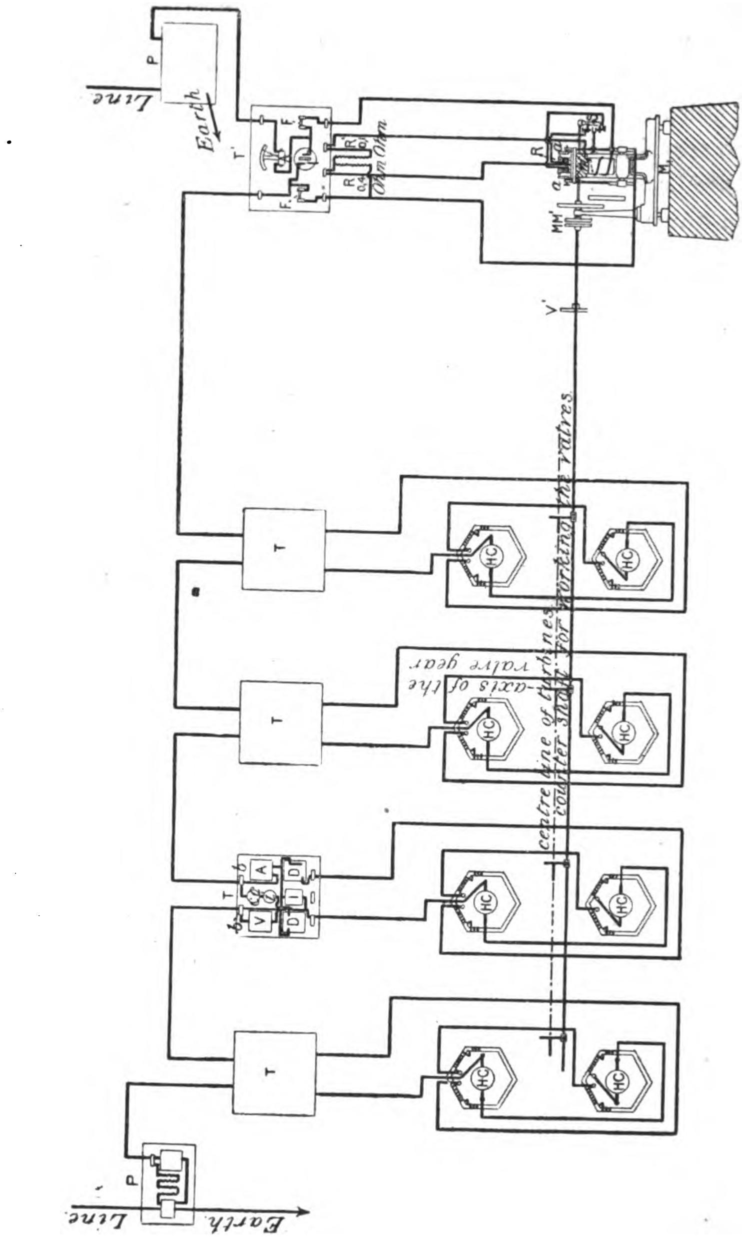


Fig. 5.

registering instruments, but simply read and plotted down at regular intervals of time. It may be seen that variations of several thousand volts take place every day instantaneously.



PLAN OF PACINOTTI STATION.

We will now see what provision has been made to maintain the current constant notwithstanding these variations.

The exciter is driven by a special turbine, the moving parts of both the turbine and the dynamo being very light, in order that they may follow very closely all the variations due to changes in the opening of the valve. The speed of the turbine varies over a very wide range, and in order to maintain throughout this range a magnetic field sufficiently powerful in the exciter, the exciter is itself separately excited by means of another dynamo used also for lighting the station.

The turbine is provided with the automatic governor described above, but its conical pendulum has been taken out and replaced by a powerful vertical solenoid, which by its action keeps in equilibrium a soft iron core weighing about 15 kilogrammes, and directly connected to the slide valve. This core is kept continuously in rotation by means of a cord driven by the turbine itself. This rotation ensures the perfect mobility of the core and the slide valve. A small oil dash-pot damps sudden movements and does not allow exaggerations in the regulation. A spring and a movable counterpoise permit the adjustment of the speed while working.

The general current passes through the solenoid. When this current increases, the core is lifted up, and, carrying along with it the slide valve, occasions a more or less partial closing of the turbine's valve; that action takes place instantaneously, and reduces the speed of the turbine, and, consequently, the exciting current. If, on the contrary, the current falls *below* the normal, the core is lowered and opens the valve of the turbine, and the turbine revolves more rapidly, thus increasing the excitation.

Pacinotti Station.

T	Wooden switch-boards insulated from the ground.
A	Ammeter.
V	Voltmeter.
D	Automatic switch acting by excess of voltage.
I	Short-circuiting device.

i	Small switch for the voltmeter.
c	Fusible cut-out for the voltmeter.
b	Terminals.
HC	Dynamos.
M_1	Electric governor with double commutator.
R	Relay.
a	Armatures changing the direction of the current, and enabling it to be sent in one or in the other of the windings.
S	Motor relay.
m	Terminal connected to the iron core of the motor.
T′	Switch-board for the motor.
F	Plugs for insulating a group from the general circuit.
MM′	Ebonite coupling.
V′	Hand wheel for regulating the speed of the turbines.
P	Lightning discharger.

To get rid of the difficulties, principally of insulation, experienced on account of the separate excitation, it was resolved for the third station to come back to the first device—that is to say, to obtain the variations of pressure by varying the speed of the turbine. But this had to be done automatically, in order to reduce the chances of accidents through mistakes by the mechanicians.

The station has four groups, each composed of one turbine and two dynamos absorbing 140 H.P.; the turbines have no automatic regulating arrangement, and no fly-wheels. The dynamos are self-exciting, with all their moving parts very light, as in the Galvani station. The insulation from the ground is also obtained in the same way.

The automatic regulation of the current is effected as follows:—The turbines are fitted with the Piccard relay motor, as described in the turbine's automatic speed regulator; the slide valve I of these four relay motors may be separately connected to, or disconnected from, a shaft running along the whole length of the building. This shaft is driven by a 1-H.P. electric motor. The motor has its armature wound with two windings in opposite directions. Each one of these windings has its own commutator. The

problem is, then, to act upon the motor in such a way that it revolves in the direction causing a closing of the valve when the current increases, and in the reverse direction when the current diminishes.

To obtain this result, an apparatus has been designed which acts as a very sensitive ammeter. It consists of a relay acted upon by the general current, the movable tongue of this relay being in equilibrium for the normal current. If a variation below or above the normal takes place in the general current, the relay tongue is attracted in one direction or in the other. According to the direction in which the relay tongue has been attracted, a current is sent through one or the other winding, and the motor revolves in the corresponding direction. As the regulator acts on several turbines at once, only a very short movement is required even for compensating large variations of voltage.

On account of the lightness of the moving parts of the turbines and dynamos, there is already an effect very like an automatic regulation. When the resistance of the circuit is increased by the addition of one or several motors, the current diminishing, the torque also diminishes, and the speed of the turbines increases, thus acting so as to bring back the current to its normal value. The reverse effect takes place when the current increases. The automatic regulator has thus only to correct very slight variations.

The throwing into circuit of one group, the others being already at work, is very simple. When at rest, the turbine's relay motors are disconnected from the shaft of the regulator; and, on the other hand, the switches are so arranged that the dynamos are short-circuited. The valve of the turbine is opened by hand until the dynamos generate the normal current; at that moment the dynamos are switched on to the circuit, and it is only necessary to connect the slide valve of the relay motor to the shaft of the regulator for the working to continue automatically.

The switching off is done still more easily than the switching on. The slide valve I is disconnected from the shaft of the regulator, and the valve of the turbine is closed: the dynamos

acted upon by the general current revolve backwards as motors, but then set in action the switch we have referred to, and which causes the short-circuiting of the dynamos, and severs their connection from the general circuit.

All round the dynamos and in front of the switch-boards, in the three stations we have just described, are arranged insulating floors, supported on porcelain insulators. When apparatus carrying the high-tension current are placed close to a wall, this wall is also carefuly insulated with boards, in order to render impossible accidents through one part of the circuit being accidentally connected to earth.

Distributing Conductors.

From the three stations two different circuits start, in order not to exceed a voltage of 6,000 to 8,000 volts per circuit. This relatively low voltage has been adopted on account of some parts of the circuit being underground.

The distributing circuit gives rise to the only objection which could apparently be brought against the series system. The current being kept constant, as well as the resistance of the line, the drop along the conductor is also constant, whatever be the amount of energy distributed. The result is that the efficiency of the distribution diminishes with the load. This objection, which is based upon that which takes place in the other systems of distribution, in which the drop always increases with the load, is, in fact, untenable in the case of water power.

With the series system, you lose proportionally the least when the consumption of energy is at its maximum—that is to say, when you have to transmit as much as you can of the energy available at the generating station. Is not that more logical than to lose very little when you have to deliver a very small amount of energy, and when you have, consequently, a large amount of power left unemployed?

Each circuit is formed of high-conductivity copper conductors, 9 millimetres in diameter, having a sectional area of 64 square millimetres, and a resistance per kilometre equal to 0·251 ohm. This conductor is bare through most of its length;

when passing through villages, it is carefully insulated with india-rubber. At some places, as when crossing railways, the conductor passes underground: then it consists of an armoured cable, well insulated. Much difficulty was experienced at first in obtaining cables sufficiently insulated to prevent any leakage, and this is still one of the chief difficulties of the installation.

The conductors are simply supported on wooden poles by means of oil insulators of the Johnson and Phillips pattern. The wooden poles have already been replaced in some places by metal poles, as many cases of the former breaking have been recorded, but happily without any fatal casualty occurring. On one occasion one of the poles supporting two conductors gave way, and both conductors fell on the ground and remained there for the whole night. No perturbation whatever was noticed at the generating station, and this accident did not interfere in any way with the working of the motors on the distribution.

On the poles are placed iron points, in order to stop any attempts to climb to the top and reach the conductors. The poles bear also notices, fixed on boards, and giving warning as to the danger arising from a contact with the circuit. These notices seem quite sufficient. It may be seen from the fact related above that a leakage does not interfere with the good working of the distribution; even a short-circuit cuts out only the corresponding portion of the line.

On the parts of the line where the two conductors of one circuit are fixed on the same poles, the former are simply placed one at some distance above the other. This is a rather unsound arrangement, as in case of the breaking of the top conductor a short-circuit would be produced by its falling on the lower wire. A much more reliable plan would consist in a wooden cross-piece fixed horizontally on the pole. This cross-piece should be sufficiently long to allow the insulators being placed about 2 metres apart. With this arrangement, it would be impossible to touch the two conductors simultaneously, even willingly, and the breaking of one of the conductors would only lead to its falling on the ground, and even that could be prevented by using guard wires. The actual length of the two circuits is above 100 kilometres.

Motors.

The strength of the current—45 amperes—has been chosen to enable the use of motors of 1 H.P., and even less, without having too low a pressure at the terminals, and also motors up to 60 H.P. without exceeding pressures of 1,500 volts. As a matter of fact, the motors connected to the circuit are of nearly any power between 3 H.P. and 60 H.P.

The question of supplying all these motors from the same circuit in such a way that the working of one does not interfere with the working of the others, and also in order that they might maintain a constant speed, was the knotty point.

Many engineers and electricians had already become interested in the question. Professors Ayrton and Perry and Mr. Kapp have made several suggestions for arriving at that result, but none of their devices had been tried practically.

At Genoa, the current is kept constant automatically at the generating stations; the only thing to be done is, consequently, to maintain constant also the speed of the motors. Two different methods are used, according to the size of the motors.

For motors under 15 H.P. there is a centrifugal governor throwing into the general circuit more or less of the exciting winding, according to the speed attained by the motor. This would not be sufficient to bring the motor to rest in case of complete suppression of the load, on account of the remanent magnetism. This is remedied by using well-annealed wrought iron for the magnet cores, and so any exaggeration in the speed is prevented.

For large motors it was thought impossible to make sufficiently sure of the quality of the iron used in the field-magnet cores to employ the same method. The centrifugal governor, instead of simply adding or taking out part of the exciting winding, changes the points at which the current enters this winding. The result is that not only a part of the winding is of no use for producing the excitation, but, moreover, this part may possibly so act as to produce the magnetism in the direction opposite to the normal. The resultant field can be brought to nought by this process, or

even have its sign changed. This arrangement works very successfully.

The motors used are of the ordinary types of the Compagnie de l'Industrie Électrique of Geneva; they are all series wound, and with two poles under 15 H.P., four poles between 15 H.P. and 30 H.P., and six poles above 30 H.P. Their efficiency varies, according to the horse-power, from 78 per cent. to 91 per cent.

As a rule they run at a relatively low speed, and are of very good mechanical construction. They are sometimes kept in motion for months consecutively. They, as well as the generators, are provided with automatic lubrication, and carbon brushes for the commutators. These carbon brushes never require any shifting, and produce no sparking, whatever may be the changes in the load. The motors are generally placed in a special room with insulated floor and walls. The switch-board comprises a voltmeter and an ammeter (apparatus which are not very useful, and might easily be dispensed with), an automatic switch for excess of voltage, and an ordinary commutator, which acts also as a starting appliance, since, for starting a motor, you have simply to switch it on the circuit without any special precaution. The contacts have only to be arranged so as never to break the circuit.

All the motors are fitted with heavy fly-wheels to help to maintain the speed constant. The motors are under the care of skilled men belonging to the generating stations staffs, their duty being to watch over the lines, and to examine the motors about once a week. In this way the chances of accident are still further diminished, these men having a good training in the management of high-tension apparatus.

EFFICIENCY.

No measurement of the efficiency of the complete installation has ever been taken, but it may be estimated very exactly from the data we possess of the efficiency of the different parts composing the installation.

The efficiency of the whole of the motors can be quite safely

taken at 85 per cent.; this being rather under the real efficiency, as the large motors are in the majority.

The drop on one circuit being approximately 500 volts, and the maximum voltage used actually 6,000 volts, the efficiency of the line is 91·6 per cent. at full load.

The efficiency of the dynamos was guaranteed at 90 per cent., and when measured was found to be 91 per cent.

So, at full load, the efficiency of the whole, from the shaft of the turbine to the axle of the motor, amounts to 73 per cent.

The Compagnie de l'Industrie Électrique, contractors to the work, had guaranteed 70 per cent.

We do not think it would have been possible with any other system to obtain such a high efficiency, and this is much to the credit of M. Thury, who has designed and carried out the whole work.

The cost of the hydraulic power was from 5d. to 2d. per H.P.-hour, according to the size of the motor. The cost of the H.P.-hour supplied electrically varies from 1d. to ½d., according to the size of the motor and the hours of utilisation.

These figures apply to the motive power distributed along the Polcevera and Verde Valleys; they are increased by 10 per cent. for Sampierdarena, and by 20 per cent. for Genoa.

The cost of electric plant per H.P. available on the axle of the motor at Genoa comes to a little under £10.

This installation shows that, in many cases, but principally for transmitting power, continuous currents are to be preferred to alternate currents, and give a higher general efficiency.

ABSTRACTS.

F. UPPENBORN—THE CENTRAL STATIONS OF MESSRS. SCHUCKERT & CO.: No. III.—DÜSSELDORF.

(*Electrotechnische Zeitschrift, No.* 14, 1893, *p.* 185.)

The municipal central station of Düsseldorf differs considerably from those of Barmen and Hanover, already described, in that the plant is placed outside the illuminated area, and the distribution is by means of accumulator sub-stations and a three-wire network. It has been in working without interruption since December 1st, 1891.

The station, which is designed for 40,000 30-watt lamps, is situated outside the town, and at a distance of from one and a half to two miles from the three sub-stations, which form approximately the three angles of an equilateral triangle; it contains at present four water-tube boilers of about 1,500 sq. ft. of heating surface, of which one forms a reserve. It will be noted that German practice differs enormously from English in size of the plant units, as this station is only to contain three sets (two now installed) of engines and dynamos, the former being horizontal compound tandem condensing engines of 300–400 H.P., revolving at 90 revolutions per minute, and having fly-wheels of about 13 feet diameter weighing 10 tons. The engine-room is fitted with a travelling crane capable of carrying about 10 tons, and worked by a motor. The dynamos are directly coupled to the engines, and are Schuckert's disc-armature shunt-wound machines, having an output of 350 kilowatts, being made for a maximum voltage of 400 and maximum current of 1,000 amperes. The machines are multipolar, having cast-iron "cases," on which the 14 polar projections are cast; the armatures are 10 ft., and the commutators 6 ft. 6 in. in diameter, the number of commutator sections being 800! An adjustment is provided for the armature, by means of which the pull on either side of the fields can be exactly balanced. An enormous space is taken up by these machines, the engines being 35 ft. long, the area taken up by three sets being $50 \times 90 = 4{,}500$ sq. ft. These dynamos feed into omnibus bars from which the leads are taken to the sub-stations, where they end in the charging switches. The station is lighted by means of a direct-current transformer or dynamotor, whose pressure is kept constant by an automatic regulator.

The sub-stations have batteries, one of 2,700 and the other two of 1,400 ampere-hours capacity, making a total of 44,000 lamp-hours; and in combination with one machine can run 20,000 lights. The cells feed into the three-wire system at 110 volts, 70 cells being on either side of the middle wire, out of which 28 are coupled to the regulation switches, which are of similar type to those at Hanover.

The cables from the central to the sub-stations are buried in sand 5 ft. deep, and covered by cast-iron plates, and are double throughout, so that in case of breakdown of one the other can be used temporarily alone. The feeders are 24 in number, leading from the sub-stations to the distributing network, and are made, like all the rest of the cables, of armoured double-lead-covered cable supplied by

Felten & Guilleaume, who supplied about 100 miles of mains for the work. The efficiency of the system is given at about 60 per cent.

A series of tests was made of the plant, by which it was found that the steam dynamos used 14·3 and 15·3 lbs. of steam respectively per indicated horse-power hour, being well within the guarantee; the efficiency of the cells was 80 per cent., against 75 per cent. guaranteed; the insulation resistance, guaranteed at 5 megohms, came out 7—40,000 megohms per kilometre.

In December, 1892, some 32,000 lamps were on the mains, which burned on an average 1 h. 58 m. per diem, corresponding very nearly with the usual average, and they used in this month about 50,000 kilowatt-hours, and burned 166·5 tons of coal. The figures of the first six months are equivalent to a profit of about 7½ per cent. on the capital expenditure of £117,000.

ANON.—THE FÜRSTENFELD-BRUCK ELECTRICITY WORKS.

(*Elektrotechnische Zeitschrift, No.* 16, 1893, *p.* 223.)

This power transmission is one of those which prove that the utility of electrical supply is not confined to large towns, but may be made a source of cheap light and an encouragement to industry by supplying power at a low rate even in small townships. The authorities of Fürstenfeld-Bruck have taken a mill situated at Schöngeising, about 4½ miles away, as their source of energy, and have here set up an installation on the alternate-current system. The dynamos are driven by turbines, which are furnished with hand and automatic regulators; and these machines are coupled to one main countershaft, from which the two alternators and the exciters are driven by belts. The alternators, of Brown, Boveri, & Co.'s make, are 38-kilowatt 2,600-volt machines, and are capable of being run in parallel; and the switchboard, furnished as usual, completes the plant. The long-distance conductors are two copper wires ¼ in. in diameter, supported on ordinary insulators; and on the same posts is carried the pilot wire, which serves to indicate the pressure in the secondary network, which is kept at 100 volts, and is also used for telephonic purposes. The high-pressure conductor is connected, as usual in these systems, to the transformers—about 10 in number in this case—whose secondaries feed the low-pressure network, the transformers being placed in cases filled with oil. The secondary network is above ground, for the sake of economy. A point to be noted is the use here of the Brown single-phase motor, in which a star-shaped magnet, excited by means of a commutator and brushes connected to one of the armature windings, rotates inside an ordinary alternate-current armature. The motor is started by gradually switching in the coils of the transformer on which it is run, and is said to run sparklessly and silently, to bear 100 per cent. overload with pulling up, and to require no adjustment of the brushes with varying load. The total output of the turbines is to be 180 H.P., equivalent to about 4,000 30-watt lamps at Fürstenfeld-Bruck, and the cost of plant works out at about 16s. 6d. per lamp; but it must be noted that the secondary cables are complete, and capable of being immediately coupled to any house in the town. The cost of running is estimated at 10s. per lamp-year if 2,400 lamps are burning

at once, and the charges made are reckoned on this basis, which gives for private lighting about 5¼d. per unit. At present the load consists of 70 houses burning 3,000 lamps, 7 arcs, and motors to an aggregate of 18 horse-power.

C. HEIM—SMALL ARCS AND INCANDESCENT GAS LAMPS.

(*Elektrotechnische Zeitschrift, No.* 14, 1893, *p,* 196.)

The Auer incandescent gas burner has been the cause of some perturbation in the minds of German electrical engineers, as it threatens to be a serious rival to electric light, and the author of this paper has made careful experiments with a view of obtaining data for a comparison between the two methods of illumination. The more important figures relate to the question of relative cost, as to which he concludes that when the gas burner is new it is much cheaper per candle-power. The burners have been improved since 1886 in efficiency by raising the temperature of the incandescent body; but this has had the disadvantage that the original candle-power quickly diminishes, and the "life" is shorter, this effect being much greater than in the case of electric glow lamps, so that the figures given are much too favourable to the gas lamp. The sum of the costs works out for 100 candle-power-hours, with gas at 4s. 6d. per 1,000 cubic feet, and electric energy at 9½d. per unit, at 1·83d.–1·75d. for small arcs, and 1·22d. for gas glow lamps. It must be noted, as regards these figures, that the arcs are supposed to be two in series on a 106-volt circuit, to take 1·5–2 amperes, and to burn 5 hours; and there are included renewals of carbons or incandescent material, and interest on capital outlay; the lights are supposed to be on 800 hours per year. As it has not been found too much to assume that at the end of 400 hours (the life assumed) the gas lamp is reduced to one-third of its candle-power, the average cost of running would rise to 1·85d.; that is, the cost of small arc lamps and Auer's gas glow lamp is about the same. Improvements are said to have been made which reduce the diminution in light, but the new incandescent material does not appear to be yet on the market.

— MOSELEY—PREPARATION OF ZINC ELECTRODES FOR GALVANIC BATTERIES.

(*Beiblätter, No.* 4, 1893, *p.* 350.)

The author makes his electrodes from thin zinc sheet, which is first amalgamated and then rolled into the desired shape; or of tubes fitting into one another. The purity and homogeneity of zinc so prepared is such that local action does not take place.

— GERMAIN—DRY CELL WITH CELLULOSE FILLING.

(*Beiblätter, No.* 4, 1893, *p.* 351.)

In this cell the cellulose of the cocoanut is used to take up the solution. The case is an oak box soaked in paraffin and made waterproof by an inner coating of rubber solution or tar; on the bottom is an amalgamated zinc plate, on which rests a layer of cellulose soaked hot in hypochlorous acid, and well pressed down;

then the carbon, surrounded by crystals of manganese dioxide, on which is a second layer of cellulose; and then the second zinc plate. The whole is kept tight by strong springs. The E.M.F. is 1·5 volts, and there is no waste on open circuit.

ANON.—THE CHRISTIANIA CENTRAL STATION.

(*Lumière Electrique, Vol.* 48, *No.* 15, *p.* 80.)

This station first started electric supply on December 13th last, and is capable of supplying current to 24,000 8-C.P. lamps; but provision has been made for lighting 40,000 lamps.

Power is supplied by four compound condensing engines of 600 H.P., and two smaller machines, of 260 H.P. and 130 H.P. respectively. These engines were built by G. Kuhn, of Stuttgart.

A small battery of accumulators of the Tudor type is employed; it has a total capacity of 685 ampere-hours at 240 volts, and is consequently capable of lighting 774 lamps for four hours.

The station is situated at about the centre of its district, of which the radius is about 800 metres; 12 feeders, manufactured by Messrs. Felten & Guilleaume, running out to the various distributing centres. These cables are lead-covered, and placed direct into a layer of fine sand covered over with a row of bricks.

The lengths of cable which have been laid are as follows:—

Principal cables	19,600	metres.
Distributing cables	32,500	,,
Branch cables	5,000	,,
Cables for public lighting	6,700	,,

making a total of 64 kilometres, the insulation resistance of which, after laying, was 550 megohms per kilometre.

Three Babcock-Wilcox boilers have been installed having a total heating surface of 645 square metres. The normal guaranteed capacity is 14 kilogrammes of water per square metre of grate surface in order that the three boilers may give 9,030 kilogrammes of steam per hour, and with forced draught, 11,610 per hour.

For the steam engines the makers guaranteed a normal consumption of 7·3 to 8·3 kilogrammes of steam per H.P.

Worthington feed pumps are used, the feed water passing through economisers before entering the boilers. The combustible employed is coke, of which, when working normally, 1 kilogramme should evaporate 8 kilogrammes of water, pressure of steam being 10 atmospheres.

The dynamos are of the Schuckert shunt-wound multipolar type. The two largest machines have 14 poles, and run at a speed of 110 revolutions per minute; two others have 12 poles, and run at 150 revolutions; the two smallest have eight poles, and run at 170 revolutions. Forty 14-ampere Schuckert arc lamps are placed in six streets.

The electrical energy supplied is measured on Aron and Schuckert meters, and charged for at the rate of one franc per kilowatt-hour.

A. BERGET—ON THE MAGNETIC EXPANSION OF IRON.

(*Journal de Physique*, *April*, 1893, *p.* 172.)

The author, in these researches, employed Mr. Fizeau's method, based on the interference of two reflected waves on the two faces of a thin layer of air.

This method is extremely sensitive, for a displacement of 1 mm. of one of the two faces will cause 3,300 rings to pass in the observing glass when viewed with yellow light.

The apparatus employed consists of a bobbin of insulated wire mounted on a tripod, its axis being vertical. The bar of soft iron under test is placed in the axis of the bobbin, and as it is necessary that this should be in the most uniform part of the field, it is made only 52·25 mm. long, and has a copper projection at each end turned to exactly the same diameter as the iron. The lower end of this rod is fixed to the base of the bobbin; the upper end projects beyond the top flange, and carries a disc of black glass which was worked up to a plane surface by M. Merlein. Above this disc is fixed a plano-convex lens of 40 cm. focal length. It is between the plane face of this lens and the black glass that the rings are produced. These rings are observed in the following way:—A Bunsen flame burning with bromide of sodium sends yellow light on to a small prism. The light then passes into a larger prism placed vertically above the plano-convex lens, and by the combined use of this larger prism and a telescope these rings are easily observed, a number of points being engraved on the face of the plano-convex lens. The following is a series of results obtained by the above method:—

Intensity of Field.	Number of Rings displaced.	Increment in Length.
49	0·85	0·000255
104	1·40	0·000412
135	1·50	0·000444
150	1·60	0·000467
160	1·62	0·000473
177	1 66	0·000483
190	1·68	0·000495
209	1·73	0·000509
238	1·80	0·000530
410	1·89	0·000556
450	1·91	0·000562

The curve obtained by plotting these values resembles the ordinary magnetism curve connecting B and H.

Directly the field is excited in the bobbin the rings are seen to displace, and by knowing this displacement the specific elongation of the bar can be deduced, the exact value of the field being obtained by using an exploring coil with a ballistic galvanometer. The author points out that the displacement cannot be due to the elongation of the bar through thermal causes, as the displacement is in all cases instantaneous.

The author next intends making experiments on transverse magnetisation by employing the above method.

W DE FONVIELLE—MEASUREMENTS OF EARTH CURRENTS AT THE ST. MAUR OBSERVATORY.

(*Lumière Electrique*, *Vol.* 48, *No.* 14, *p.* 8.)

As is well known, certain currents exist in telegraph wires which are similar in all respects to those produced by the battery. The first attempt to investigate the nature of these currents was made by the Greenwich Observatory 30 years ago.

Sir George Biddel Airy had constructed two special telegraph lines for recording these earth currents. The lines terminated at each end in a copper earth plate. For the sake of economy the telegraph posts belonging to the South Eastern Railway were employed. Unfortunately these lines are not at right angles to one another, and have some arbitrary direction.

The azimuth of the line with the first two earths makes an angle of 46° N.W. of the magnetic meridian, whereas the azimuth of the second makes 50° with the N.E. The northern angle of the two lines is then 96°, whilst the two adjacent angles make only 84°.

The observations made at Greenwich, lasting over a long period, yielded most important results.

Present records are, however, entirely vitiated by disturbances, for which the South London Electric Railway are responsible, owing to the fact that earth has been employed as a return.

It has for some time been Mr. Mascart's desire to employ lines of the same kind in the Parc St. Maur, to complete the system for registering variations of magnetism and atmospheric electricity; and to continue that work in France which had unfortunately been stopped in England.

Definite positions and directions were chosen for the earth plates and lines. Observations were then to be obtained, first, to be comparable with declination variations, and, secondly, with the horizontal force. In order that these comparisons should be perfect, it was considered advisable to erect a circular overhead circuit quite free from earth currents, to be only acted upon inductively, and yielding results comparable with the vertical force.

These lines have been run on posts belonging to a neighbouring railway company, the only objection being that there are telegraph and telephone lines in close proximity; the corrections for these, however, will not be great enough to seriously affect final results. Airy, at Greenwich, employed very sensitive galvanometers having 800 turns with 7·3 ohms resistance, carrying a mirror, and reflecting a beam of light on to a sheet of sensitised paper. It is in the same way that Mr. Mascart records the three curves parallel to one another on the same sheet; the galvanometers employed being of the Deprez-D'Arsonval type, which have the very great advantage for observations of this kind of being dead-beat. Both at Greenwich and at the Parc St. Maur shunts are used on the galvanometers. The curves obtained are read to a scale of volts, a standard deflection being obtained from a constant E.M.F. cell.

The record given by the current from the overhead circuit is a line of which the inflections are weakly defined, being mostly formed of continuous variations indicating exceedingly rapid and isochronous oscillations.

This system has not been long enough in use to permit of many remarks on the results obtained; but it has been specially noticed that there exists a great similarity between the current curves of the line lying east-west and the variations of the declination. The inflections are parallel, and the maxima and minima are quite synchronous. This fact was communicated to Greenwich Observatory. Mr. Ellis had, however, observed from the mean of a great number of observations that the oscillations of the magnet lagged slightly behind the variations of the current. Mr. Moureaux considers that the system at present in use does not allow of any definite conclusion to be drawn from so small a value. He considers that, instead of quantity, the sensitiveness of the records should be increased, and suggests that the photographic papers on which these records are made should travel at a greater rate, and that the amplitude of the vibrations should be increased.

C. THWING—A PHOTOGRAPHIC METHOD OF REPRESENTING A MAGNETIC FIELD.

(*Journal de Physique, April*, 1893, *p.* 191.)

In a room free from actinic light, a photographic dry plate is placed where it is desired to record the disposition of magnetic lines of force. Iron filings are then sprinkled over the surface of the plate in the usual way, and actinic light is allowed to act upon it from above. The light is then removed, and the filings dusted off. The plate, when developed and fixed, will give a negative image.

ANON.—THE FORMATION BY ELECTROLYSIS OF POROUS LEAD FOR ACCUMULATORS.

(*Lumière Electrique, Vol.* 48, *No.* 15, *p.* 76.)

This is known as the Correns process. A lead grid is taken, and the spaces filled up with a mixture of lead salts, sulphuric acid, and gelatine. The grid, with its spaces filled up with lead sulphate, is treated as an anode in a bath of sulphuric acid, with a cathode of carbon covered with mercury, until the substance in the spaces becomes converted into lead amalgamate. This porous lead is then used for filling up lead grids in the ordinary manner.

E. ARNOLD—NON-SYNCHRONOUS MOTORS FOR ORDINARY ALTERNATE CURRENTS.

(*Elektrotechnische Zeitschrift, No.* 18, 1893, *p.* 256.)

The author considers that to Elihu Thomson and Nikola Tesla must be given the credit of having laid the foundations of the design of a practical alternate-current motor, but numerous difficulties have retarded its development, and only the recent experience obtained with polyphase transmission of power has enabled engineers to construct a satisfactory non-synchronous single-phase motor. The development of the machine is summed up by the author briefly as follows. Elihu Thomson was the first to show by experiment that a conductor placed in an alternating magnetic field, and made capable of rotation, will, if either started or placed unsymmetrically in the field, tend of itself to increase its speed of rotation and also try to keep up this speed—that is, exert a torque; and if there is substituted for the simple massive conductor an armature of laminated iron having

on it short-circuited windings of copper bars, the effect is much increased, the armature attains a nearly synchronous speed, and can be made to do work without much decrease in angular velocity. A motor built on these lines consists of an inducing and an induced system of winding, the latter being usually made capable of rotation, and it becomes a short-circuited armature, having no collector, commutator, or brushes; and its direction of rotation depends merely on the direction of the necessary starting moment.

In connection with starting devices, the author draws attention to an important patent of Tesla's (February, 1889), which describes a method of starting by means of a rotatory field, produced by dividing the sets of exciting coils into two parts having a different self-induction, and which at starting are placed in parallel; the currents in them are different in phase, and cause, therefore, a rotating magnetic field by means of which the motor starts. When it has attained a certain speed the fields can be again coupled in series, and the motor works as a single-phase machine. Tesla's motors were not very successful; but this was because his magnetic design, having very sharply projecting poles, is not suitable for this class of work, and his armature also was not of the right kind. To work well, these motors should have very low magnetic resistance, and the notches in the armature should be small; indeed, the winding through holes, as in the Wenström design adopted by Mr. Brown, is very suitable. The conditions which hold for polyphase current motors have been found to apply also to single-phase machines, and the Œrlikon Company have thus arrived at a design for non-synchronous motors, of which the author gives details, and which are in the market in sizes of from $\frac{1}{16}$ to 12 H.P., while a large 250-H.P. machine has for some time been running at the Œrlikon Works. The field or inducing coils of this machine are wound as a "Gramme" ring, and a neat method of starting is described in which this winding is connected to a fixed commutator just as if it were a direct-current armature, and the alternator is coupled to the brushes, which are capable of rotation round the commutator, moved by hand. If these brushes are rotated when the machine is switched on, a rotary field is produced, and the motor starts; the commutator can be on the switch-board near the switch, and the brushes can be left in any position when the motor has started. The Tesla method does for small motors, if an initial speed be imparted to the machine; but it is apt to take too much current. A third method of starting, which is that used by Mr. C. E. L. Brown, but fully described in a patent specification of the Œrlikon Company last year, is to have the armature arranged as a Gramme ring and connected like a direct-current machine to a commutator, and to send the current at first through both armature and fields; the motor then works like a series direct-current motor, and the current is controlled by means of a resistance. The motor thus started soon attains a nearly synchronous speed, and the armature windings are then short-circuited by means of a ring which is pressed on so as to make contact with all the commutator sections.

The following details will give an idea of the Œrlikon motors referred to, illustrations of which are given in the article:—Œrlikon alternate-current motor for 110 volts and 65 ∿. Output, 12 H.P.; speed, 1,170 revolutions per minute; watts taken by motor, 10,600; efficiency, 82 per cent.; length, 2 ft. 10 in.; breadth, 4 ft.; height, 2 ft. 9 in.; weight, 120 lbs.

L. NEUSTADT—CONCENTRIC CABLE PHENOMENA IN ALTERNATE-CURRENT WORKING.

(*Elektrotechnische Zeitschrift, No.* 18, 1893, *p.* 253.)

The paper contains a discussion of the reasons for a phenomenon observed by the author in a large high-pressure network comprising about 53 miles of cable, which was that when a section of cable was switched in or out while running, or if a fuse blew, the insulation was frequently punctured. This puncture occurred even with cables of the very best insulation possible, but only when the inner conductor was coupled on first, as in making a joint, or conversely if the outer conductor were first broken, and if the section in question had very lightly loaded transformers on it. The explanation is found by means of formulæ and diagrams, which must be consulted by anyone interested in the subject, but which give the following numerical example:—Suppose a frequency of 43 ∿, an alternator at 2,000 volts, a cable network of 50 miles to which is coupled a section of about a mile containing transformers of 20 kilowatts aggregate output, having no secondary load: then the voltage between the two conductors will rise to 5,300, and that between the outer layer and earth (or sheathing) to about 5,400 volts. And if the curve given by the alternator be very different from a sine curve, the volts may rise still more, and a further increase takes place on account of the saturation of the cores of the transformers by the large current.

The author concludes that an arrangement of switching should be adopted which should render it impossible to have the inner conductor on alone.

J. PRECHT—ABSOLUTE MEASUREMENTS ON THE DISCHARGE OF ELECTRICITY FROM POINTS.

(*Wiedemann's Annalen, Vol.* 49, *No.* 5, *p.* 150.)

The influence of points in discharging electricity has been much overestimated. In Dvorák's experiment it was found that a needle so finely pointed as to appear quite sharp under a magnification of 200 diameters was found to be capable of approaching within $\frac{1}{10}$ mm. of a charged electroscope without discharging it The author found that, generally speaking, points can be highly charged without discharging. For instance, the points of lightning conductors can only equalise potential between clouds and earth when a pressure of 15,000 volts is attained at the point; and even with extremely fine points the value reaches 2,500 before a constant flow is set up, though spasmodic discharges may take place at lower potentials. Much dust or gas in the neighbourhood prevents discharge, but ultra-violet radiation is found to help it. A bundle of points requires a higher potential for quiet discharge than the individual members of the group.

As regards the use of lightning conductors to discharge clouds quietly and so prevent the generation of a potential difference high enough for a spark—a property which is spoken of as more or less important—the author is of opinion that this action is quite unimportant, owing to the rapidity with which these potentials are reached.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Month of MAY, 1893.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

L. Neustadt—Phenomena connected with the Use of Concentric Cables in Alternate-Current Working.—*E. T. Z.*, No. 18, 1893, p. 253 (I.).

E. Arnold—Non-Synchronous Motors for Ordinary Alternate Currents.—*E. T. Z.*, No. 18, 1893, p. 256 (I.).

K. Friedrich—New Machines for Precise Electric Work.—*E. T. Z.*, No. 18, 1893, p. 259, No. 19, p. 272 (I.).

Anon.—The Importance of Electric Traction for Rapid Transit in Towns.—*E. T. Z.*, No. 18, 1893, p. 265.

H. Kratzert—A New Alternate-Current System.—*E. T. Z.*, No. 19, 1893, p. 269 (I.).

M. Corsepius—Magnetic Reactions in Dynamos and Motors.—*E. T. Z.*, No. 19, 1893, p. 270.

J. Sahulka—The Use of Condensers in Alternate-Current Working.—*E. T. Z.*, No. 20, 1893, p. 281, No. 21, p. 298 (S. I.).

C. Brown—The Brown Motor and Herr von Dolivo-Dobrowolsky.—*E. T. Z.*, No. 20, 1893, pp. 283, 285.

M. von Dolivo-Dobrowolsky—In reply.—*E. T. Z.*, No. 20, 1893, p. 285.

Anon.—Electric Train Lighting.—*E. T. Z.*, No. 20, 1893, p. 286.

— Behn-Eschenburg—An Alternate-Current Motor capable of Regulation.—*E. T. Z.*, No. 21, 1893, p. 300 (I.).

Anon.—Munich Street Lighting.—*E. T. Z.*, No. 21, 1893, p. 304.

Anon.—Parallel Running of Alternators.—*E. T. Z.*, No. 21, 1893, p. 307.

W. Wedding—The Electric Arc Light and Incandescent Gas Lamp.—*E. T. Z.*, No. 21, 1893, p. 310 (I.).

G. Richard—Arc Lamps.—*Lum. El.*, vol. 48, No. 18, p. 213 (S. I.).

Anon.—The Mitchell Heater.—*Lum. El.*, vol 48, No. 18, p. 229 (I.).

P. Marcillac—The Marseilles Electric Tramways.—*Lum. El.*, vol. 48, No. 19, p. 251, No. 20, p. 305 (I.).

W. de Fonvielle—The Japanese Ship Railway.—*Lum. El.*, vol. 48, No. 19, p. 275 (I.).

Ch. Jacquin—Polyphase Transmission of Power at Heilbronn.—*Lum. El.*, vol. 48, No. 20, p. 301, No. 21, p. 370 (S. I.).

Richard—Incandescent Lamps.—*Lum. El.*, vol. 48, No. 20, p. 317 (I.).

J. P. ANNBY—Distribution of Electric Energy: The Three- and Five-Wire Systems.—*Lum. El.*, vol. 48, No. 20, p. 323 (S. I.).

M. VON DOLIVO-DOBROWOLSKY—Rotary-Field Motors of the Allgemeine-Elektricitäts-Gesellschaft.—*Lum. El.*, vol. 48, No. 20, p. 328 (I).

G. RICHARD—The Mechanical Applications of Electricity.—*Lum. El.*, vol 48 No. 21, p. 358 (S. I.).

F. GUILBERT—The Œrlikon Alternate-Current Motors.—*Lum. El.*, vol. 48 No 21, p. 366 (I.).

ANON.—Magnetic Coupling for Electric Traction.—*Lum. El.*, vol. 48, No 21, p. 382 (I.).

DYNAMO AND MOTOR DESIGN.

P. BOUCHEROT—The Influence of Iron on the Form of the Sine Function Curves in Dynamos.—*Lum. El.*, vol. 48, No. 18, p. 206 (I.).

MAGNETISM.

A. HEYDWEILLER—On Villari's Critical Point in Nickel.—*Phil. Mag.*, No. 216, 1893, p. 469.

A. VERNER—Suggested Explanation of the Phenomenon of Rotary Magnetic Polarisation.—*Jour. de Phys.*, vol. 2, 1893, p. 221.

— CHASSAGNY—On the Influence of Longitudinal Magnetisation on the Electromotive Force of an Iron-Copper Couple.—*Lum. El.*, vol. 48, No. 21, p. 395; *C. R.*, vol. 116, No. 18, p. 977.

INSTRUMENTS AND MEASUREMENTS.

M. LEVY—A Note on the Use of the Differential Galvanometer.—*W. A.*, No. 5, 1893, p. 196; *E. T. Z.*, No. 18, 1893, p. 255.

— KAPSEL—Siemens & Halske's Invariable Current-Indicators with Permanent Magnets.—*E. T. Z.*, No. 18, 1893, p. 265 (I.).

ANON.—Apparatus for Signalling Time of Departure of Trains.—*E. T. Z.*, No. 19 1893, p. 276 (I.).

ANON.—Variation of the Resistance of Gutta-Percha with Temperature.—*Lum. El.*, vol. 48, No. 18, p. 225 (I.).

A. RIGHI—On the Distribution of Potential in an Electric Field in Rarefied Air —*Lum. El.*, vol. 48, No. 18, p. 237, No. 19, p. 291 (I.).

ANON.—The Wilson-Salmon Block System.—*Lum. El.*, vol. 48, No. 19, p. 277 (I.).

N. H. GENUNG—Improvements in the D'Arsonval Galvanometer.—*Lum. El*, vol. 48, No. 19, p. 278 (I.).

E. VILLARI—A Modification of the Thomson Electrometer.—*Lum. El.*, vol. 48, No. 20, p. 329 (I.).

ANON.—Kapp Meter.—*Lum. El.*, vol. 48, No. 21, p. 376 (I.).

ANON.—Instruments and Methods for Magnetic Measurements.—*Lum. El.*, vol. 48, No. 21, p. 377 (I.).

D. KORDA—Measurement of the Difference of Phase of Two Sinusoidal Currents. —*Lum. El.*, vol. 48, No. 21, p. 394.

ELECTRO-CHEMISTRY.

ANON.—Electrical Production of Chlorine and Caustic Soda.—*E. T. Z.*, No. 21, 1893, p. 306.

ANON.—Cabarros' Dry Cell.—*Lum. El.*, vol. 48, No. 18, p. 230 (I.).

W. HAMPE—The Simultaneous Deposition of Copper and Antimony by the Electric Current.—*Lum. El.*, vol. 48, No. 19, p. 277.

ANON.—The Weston Standard Cell.—*Lum. El.*, vol. 48, No. 21, p. 374 (I.).

ANON.—The Potier Process of Electro-Deposition on Glass, Porcelain, &c.—*Lum El.*, vol. 48, No. 21, p. 375.

H. MOISSAN—Work with the Electrical Furnace.—*C. R.*, vol. 116, No. 22, p. 1225.

TELEGRAPHY AND TELEPHONY.

ANON.—Telephoning on Telegraph Wires.—*E. T. Z.*, No. 18, 1893, p. 264.

ANON.—Mix and Genest's Conversation-Timer for Telephone Stations.—*E. T. Z.*, No. 19, 1893, p. 271 (I.).

K. STRECKER—The Use of Storage Cells on Telegraph Lines.—*E. T. Z.*, No. 20, 1893, p. 287 (I.).

ANON.—Haynes's Long-Distance Telephone.—*Lum. El.*, vol. 48, No. 18, p. 232.

G. RICHARD—The Gray Telautograph.—*Lum. El.*, vol. 48, No. 19, p. 267 (I.).

ANON.—The Eichberg Adjustable Insulator.—*Lum. El.*, vol. 48, No. 20, p. 328 (I.).

— GANSAUGE—Note on Static Induction in Overhead Telegraph Lines.—*Jour. Tel.*, vol. 17, No. 5, p. 101.

E. ZETSCHE—The Jamolet Multiple Switch for Telephone Exchanges.—*Jour. Tel.*, vol. 17, No. 5, p. 109.

ANON.—Telegraphs and Telephones in Sweden in 1891.—*Jour. Tel.*, vol. 17, No. 5, p. 113.

THEORY.

T. H. BLAKESLEY—On the Differential Equation of Electric Flow.—*Phil. Mag.*, No. 216, 1893, p. 419.

E. MERCADIER—On Systems of Electric Units.—*C. R.*, vol. 116, No. 18, p. 974.

C. HUC—On the Material Nature of Electricity.—*C. R.*, vol. 116, No. 21, p. 1212.

M. VASCHY—A General Property of Fields having a Potential.—*C. R.*, vol. 116, No. 22, p. 1244.

ATMOSPHERIC AND STATIC ELECTRICITY.

H. EBERT and E. WIEDEMANN—On Electric Discharges: Production of Electric Oscillations, and the Relation of Discharge Tubes to them (Conclusion).—*W. A.*, No. 5, 1893, p. 1.

H. EBERT and E. WIEDEMANN—Experiments in Electro-dynamic Screening and Electric Shadows.—*W. A.*, No. 5, 1893, p. 32.

J. PRECHT—Absolute Measurements on the Silent Discharge of Electricity from Points.—*W. A.*, No. 5, 1893, p. 150.

J. RITTER VON GEITLER—On Reflection of Electric Waves.—*W. A.*, No. 5, 1893, p. 184.

C. P. STEINMETZ—Descriptive Phenomena in Dielectrics under High Electric Potentials.—*E. T. Z.*, No. 18, 1893, p. 248 (I.).

VARIOUS.

J. PERRY and OTHERS—On Liquid Friction.—*Phil. Mag.*, No. 216, 1893, p. 441 (I.).

BERLIN REICHSANSTALT—Proposals for the Legal Determination of Electric Units.—*E. T. Z.*, No. 18, 1893, p. 245.

ANON.—Eglinger's Constant Battery for Microphones.—*E. T. Z.*, No. 19, 1893, p. 277 (I.).

M. VON DOLIVO-DOBROWOLSKY—On the Question of the Legalisation of Electric Units.—*E. T. Z.*, No. 21, 1893, p. 295.

G. GOURÉ DE VILLEMONTÉE—Equality of Potential of Two Electrically Deposited Layers of the same Material.—*Jour. de Phys.*, vol. 2, 1893, p. 213; *Lum. El.*, vol. 48, No. 19, p. 285.

— MASSIN—On Measurements of Capacity and Self and Mutual Induction on Aërial Lines.—*Jour. de Phys.*, vol. 2, 1893, p. 236.

D. HURMUZESCU—Vibration of a Wire traversed by a Direct Current.—*Jour. de Phys.*, vol. 2, 1893, p. 237; *Lum. El.*, vol. 48, No. 21, p. 356.

P. F. MOTTELAY—Chronological History of Electricity.—*Lum. El.*, vol. 48, No. 18, p. 220, No. 20, p. 320 (S.).

ANON.—Sgrimanski's Depolariser.—*Lum. El.*, vol. 48, No. 18, p. 228.

H. HELMHOLTZ—On Kathode Rays in Gases at Atmospheric Pressure and in a High Vacuum.—*Lum. El.*, vol. 48, No. 18, p. 241 (I.).

J. BLONDIN—The Telephote.—*Lum. El.*, vol. 48, No. 19, p. 259 (I.).

D. KORDA—Multiplication of the Number of Periods of Sinusoidal Currents.—*Lum. El.*, vol. 48, No. 20, p. 345.

A. DITTE—Contribution to the Study of the Leclanché and other Cells.—*C. R.*, vol. 116, No. 18, p. 984, No. 20, p. 1128.

A. RENAULT—M. Ditte's Researches on the Leclanché and other similar Cells.—*Lum. El.*, vol. 48, No. 21, p. 351 (I.).

R. COLSON—On Electric Interferences produced in a Liquid.—*C. R.*, vol. 116, No. 19, p. 1052.

H. BAGARD—On the Inversion of the Peltier Phenomenon between Two Electrolytes.—*C. R.*, vol. 116, No. 20, p. 1126.

CH. BOREL—Dynamical Phenomena due to Residual Charge of Dielectrics.—*C. R.*, vol. 116, No. 21, p. 1192.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

TABLE OF CONTENTS.—No. 108.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXII.	1893.	No. 108.

The Two Hundred and Fifty-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 9th, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting of May 25th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Lawrence H. S. Ellson.
James Walter Grimshaw.
Robert Clay Jones.
Henry Lea.
Evelyn E. Porter.

From the class of Students to that of Associates—

Sydney Hopwood Blake.
James Hugh Garnett Gandy.
T. E. Slaughter.

Donations to the Library were announced as having been received during the recess from the Astronomer Royal; Mr. F. B. Behr; the Director-General of Telegraphs, India; Messrs.

Charles Griffin & Co.; Mons. E. Jacquez; the Minister of Posts and Telegraphs, Rome; the Radcliffe Library (Oxford); Mr. F. C. Webb; Nikola Tesla, Foreign Member; C. H. W. Biggs, A. Fahie, Druitt Halpin, Professor Andrew Jamieson, Professor A. B. W. Kennedy, W. Perren Maycock, Professor Henry Robinson, Sir David Salomons, James N. Shoolbred, Alexander Siemens, A. A. Campbell Swinton, and Sir Charles Todd, Members; C. C. Hawkins and F. Wallis, L. Newitt, J. T. Niblett, J. Munro, and R. W. Weekes, Associates.

The Secretary also announced that Sir David Salomons had presented the Institution with a Library clock, and Mr. A. Stroh a framed lithographic portrait of the late Mr. Edward Graves. To all these donors the thanks of the meeting were unanimously accorded.

The following paper was then read:—

THE ELECTRICAL TRANSMISSION OF POWER FROM NIAGARA FALLS.

By Professor George Forbes, F.R.SS. (L. & E.), Member.

TABLE OF CONTENTS.

Introductory.

Professor Forbes. I had originally intended the title of this paper to be "The Transmission of Power by Electricity," but I found on writing it that it was inevitable that the work on which I am

engaged as electrical consulting engineer at Niagara Falls should take a very prominent part in its substance; but the paper does still deal with the general question. Professor Forbes.

On the 14th of December, 1892, I read before the Society of Arts a paper on the utilisation of Niagara Falls, in which the general plan of operations was described. But at that time the electrical developments were not sufficiently advanced to enable me to treat of that part of the subject. A reference to that paper will prevent the necessity of my wasting time in describing in detail the civil engineering and hydraulic arrangements. The object of the present paper is to put before this Institution the circumstances under which this great problem has been attacked, and the views which have been arrived at, and also to describe, so far as any work has been done, the electrical machinery to be installed.

The utilisation of the Falls of Niagara has long been a favourite theme of engineers, but it was not until the transfer of power by electricity became feasible, that this could enter very satisfactorily into the region of practical engineering. The matter is now in the hands of one of the most powerful combinations of New York capitalists which has ever been formed. Under their auspices the matter was, for the first time, thoroughly investigated from an engineering, financial, and commercial point of view. Rights were then acquired, and companies formed. The Cataract Construction Company does the engineering work, and will then hand it over to the Niagara Falls Power Company. The Land Development Company builds a whole village on the extensive lands acquired, which are shown on Fig. 1, where the thick lines enclose the company's property. The Niagara Junction Railway Company constructs six miles of terminal railway, A, to connect all the factories, as they are built, with the railways in the neighbourhood. Rights of way are obtained in many directions, and also over the Erie Canal, which connects the Niagara and Hudson Rivers. Allied companies are formed to develop power to all the cities within reach. Roads are made, leases of land granted, factories built, and the engineering works taken in hand by the Cataract

Professor Forbes.

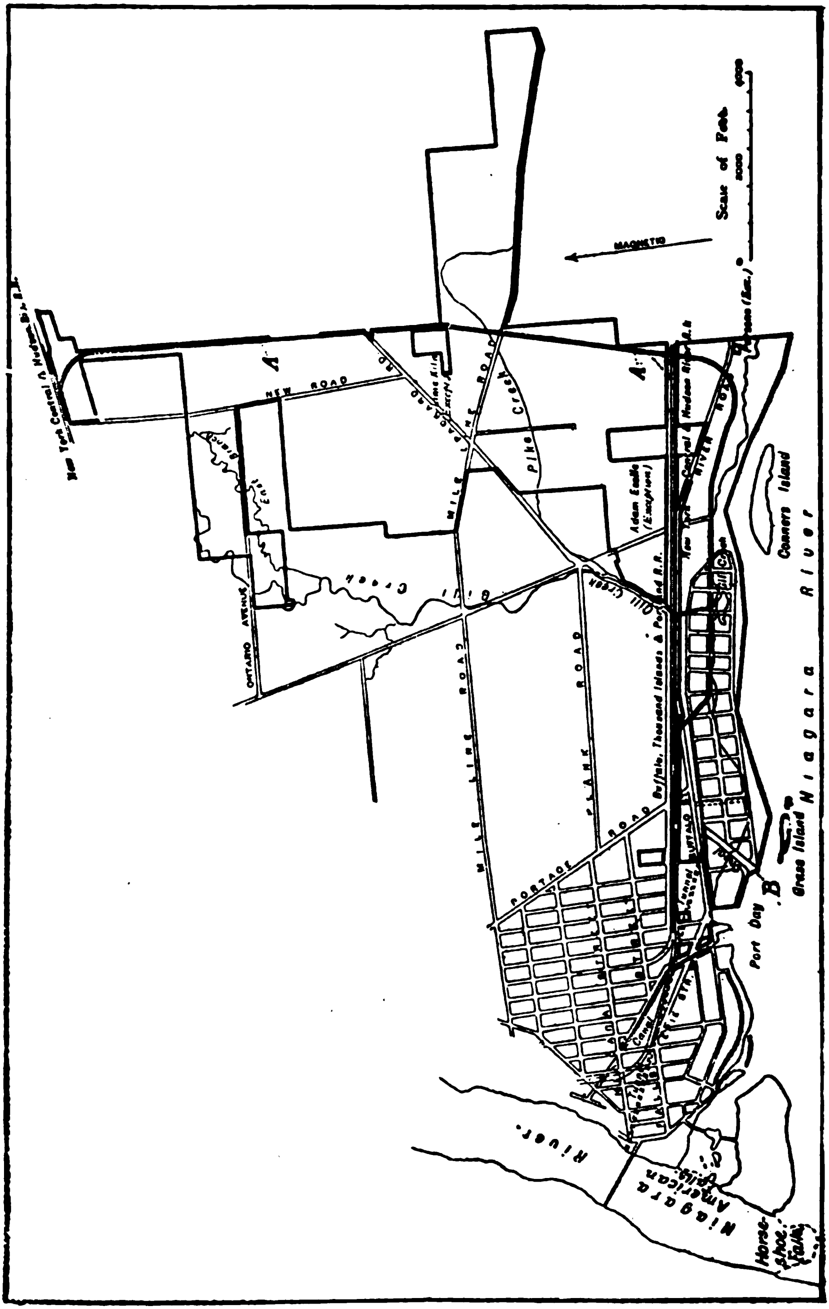


FIG. 1.—Lands of the Niagara Falls Power Company (the Thick Lines enclose the Company's property).

Professor Forbes.

Construction Company. It is with this latter that we have chiefly to deal. Mr. E. D. Adams is the president, Mr. F. L. Stetson the first vice-president, and Mr. E. A. Wickes second vice-president; Mr. W. B. Rankine is secretary and treasurer; Mr. G. B. Burbank is the chief engineer; and Dr. Coleman Sellers acts both as consulting engineer to the Cataract Construction Company and president of the Niagara Falls Power Company, while undertaking at the same time many of the duties of resident engineer. The waters from the river are taken in at a point about a mile and a half above the Falls by a canal, B, from which it is led, by channels, into a long slot in the ground, going to a depth of 200 feet. In this slot are iron pipes, or flumes, down which the water is carried to the turbines, and the waste water is carried through a tunnel—shown by dotted lines in the figure—for a distance of 7,000 feet to a point a few hundred yards below the American Falls, where it discharges into the lower river. Each turbine is of 5,000 horse-power, revolves at a speed of 250 revolutions per minute, and is mounted on a vertical shaft. Above it there is a shaft extending in a vertical direction to the surface of the ground, on which a power house is built, and the revolving part of the dynamo is placed directly upon the top of this shaft. The present paper may be divided into two distinct parts—first, the plans of working, of which, as their consulting electrical engineer, I have recommended the adoption; and, second, the design of the machinery which is to be used. In neither of these two parts can I claim to have done anything of great novelty or originality. I have simply collected together the results of experience which are available to all engineers, and in any advice that I have given I have simply followed the logical conclusions that were to be derived from this past experience; and in this I have been benefited by the courtesy and kind assistance of professional men, inventors, and manufacturers in both continents, who, almost without exception, have put their experiences at my disposal. If I have been the instrument to put before the company any designs of value, it has been to a great extent as the assembler of ideas which have originated as often in the minds of the engineers and draughtsmen who have given me

Professor Forbes. their assistance, as in my own. I wish especially to express how much the company is indebted to Professor Coleman Sellers, one of the most distinguished mechanical engineers in the United States, who has assisted me throughout with his advice and suggestions, and who may be said to have originated the form of bearings and their supports which are to be used in the Niagara dynamos. I have also received valuable suggestions from my colleagues, Professor Unwin and Colonel Turrettini, and also from the draughtsmen who have assisted me, and from numerous friends in our profession.* I have been desired by the president of the company to take this opportunity of stating some of the events which have culminated in the present satisfactory position of the company with regard to its contracts for machinery.

Relative Advantages of Direct and Alternating Currents.

A few years ago it would have been considered necessary to devote a considerable amount of space to the discussion of the relative merits of direct and alternating currents. During the last two years, however, opinions have advanced so steadily in the direction of preferring the alternating current for the transfer of power to any considerable distance, that it is not necessary to give much time to this question. One of the chief difficulties in connection with using the direct current is that it is necessary for getting the best results, to connect a number of dynamos in series, and also to put the motors at the receiving end in series with each other. This involves the insulation of each dynamo and motor from the earth—a requirement which can in some cases be attained, but which in a general system of distribution is apt to be attended with difficulty, and perhaps with danger. The best case of the kind which has been put in practice is that

* I must mention the names of the following draughtsmen who have worked on the dynamo designs for me. In England, Mr. F. M. Weymouth was chief draughtsman, assisted by Messrs. A. Wilson, R. H. Simpson, G. E. Groom, F. Willby, W. L. Hamilton, W. M. Williams, and W. H. Hudson. In America, Mr. Baumann was chief draughtsman, assisted by Messrs. Steen and Jacobson; Mr. Vogel also worked for me there.

of Genoa, and we gave it the most serious attention, but came to the conclusion that, for our purpose, it was undesirable. Professor Forbes.

The facility which the stationary transformers used with the alternating current give for varying the pressure according to the requirements of economy or safety, is one great feature in favour of the alternating current. The question was considered in all its bearings with the utmost care, and it was not until the month of May, 1893, that the board of directors passed the resolution to adopt the alternating current both for their distant transmission and also for the works nearer to the power house. It was proposed, indeed, by some manufacturers to distribute the current within a radius of a mile or two at 700 or 800 volts, and one firm proposed to do this by means of a direct current. Had this plan been accepted, we should have arrived at the surprising result that, by using the continuous current at 700 volts within a mile or two, and high-pressure alternating current for more distant places, it would have cost more to produce a horse-power at Niagara Falls than at Buffalo.

The argument that seemed to me the most important in favour of using the direct current was, that motors for this purpose have been made in much larger quantities than alternators, and that it would be seldom necessary to build special types of machines to act as motors. This advantage seemed to me of great importance, until I realised the value of a low frequency used with the alternating current. So soon as we reduce the frequency low enough, we are able to alter a direct-current motor into a synchronising alternating motor, by the simple addition of rings placed on the commutator and electrically connected with opposite bars of it, and a brush rubbing on each of these collecting rings. I also took into consideration the question of the possibility of storing up energy in secondary batteries during the night-time, when, of course, the demands on our plant would be least, and giving it off during the daytime; but at the present time the cost of the batteries would not repay work in this direction, and it would be cheaper to make another tunnel, with wheel pits, turbines, and all the paraphernalia required to generate

Professor Forbes.

current, than to go to the expense of putting down the large amount of lead which is required for these batteries. With the alternating current we have the choice of a considerable number of motors of different types. We have the synchronising motor, the series-wound motor with laminated field and commutator, the multiphase motors which attracted so much attention at the Frankfort Exhibition of 1891, and a host of single-phase motors which have been developed by various inventors, but which have never been placed on the market, because of the high frequency which has been prevalent, with which they did not work altogether satisfactorily. These motors become immediately available if we use a lower frequency. The generation of an alternating current also permits of the use of a commutator, or rectifier, with direct-current motors.

Number of Phases.

Assuming now that it is generally agreed that the alternating current must be adopted, not only for the distant transmission, but also for the nearer work, the next point to consider is the number of phases—whether one, two, or three currents, differing in phase, should be generated. The possibility, too, of using even a greater number of currents in different phases was also considered, but it did not seem to possess advantages.

As already stated, there are many motors which are suitable for use with a single-phase, or simple alternating, current at low frequency, which start without assistance. In the workshops of nearly all the most able electricians which I have visited in the course of the last year or two, I have found such motors built upon different plans, and nearly all of them seemed to work fairly well, and promise to be very efficient at low frequency.

For all heavy work which is going on constantly, without stopping the machinery, no motor could be more suitable than the synchronising alternator using a simple alternating current, and its speed is as regular as that of the turbine which is developing the power. It seems, however, that, even if single-phase motors are going to be adopted, it would be best for the generator to be of the two-phase type, because, in this way, we get a larger

output for the same size and price of machine: the circuits may be perfectly independent, and supply separate motors. Also, the two-phase system makes the rectification or commutation of the current more easy, for use for street railways, electro-metallurgy, &c. Again, multiphase motors have the great advantage that they have been considerably utilised already, and even in small sizes have a fairly good efficiency. Professor Forbes.

With regard to the relative merits of two and three phases, several claims that cannot be supported have been put forward in favour of the latter. First, it is claimed that the saving in the copper on the line is 25 per cent. over a one-phase system, and 25 or 13 per cent. over a two-phase system, according as four or three wires are used for the purpose. I investigated this matter carefully, and arrived at the conclusion that this was not the case, and that the three-phase system had no advantage in this respect over a two-phase system with three wires. Second, it is claimed that there is a greater simplicity of wiring with three phases than with two phases; but this advantage disappears when we remember that the two-phase system can be used with only three wires, although this is not a plan which I would recommend. Third, it is claimed that any pair of the three wires may be used for a distribution of lighting. This is not the case. A two-phase system, where the circuits are completely independent, is much more suitable for the purpose, and maintains the lights at a more constant electric pressure. Fourth, it is claimed that there is a smoother starting and rotating effort with the three-phase than with the two-phase system. This was originally claimed on theoretical reasons only by Dobrowolski, but everyone who has used the type of two- or three-phase motors made by the Œrlikon Company, by C. E. L. Brown, and by the Allgemeine Electricitäts Gesellschaft, of Berlin, and others, is convinced that this alleged advantage, which seems probable enough, is purely theoretical; it is not confirmed by actual practice, and, moreover, when this fact is known, the theoretical reason for it is pretty evident.

No other claims in favour of the three-phase system over the two-phase have ever, to my knowledge, been advanced; and, as

Professor Forbes.

shown above, a very full consideration of these claims does not tell in favour of the three-phase.

I will now point out what seemed to me great objections against the use of the three-phase system, due to the fact that the three conductors are all inter-connected. First, it introduces trouble in maintaining the efficient working of the line, and in testing it; so that a higher type of electrician would be required to make these tests, and even he would have greater difficulty in finding the faults and correcting them. Secondly, when the three circuits are unequally loaded, the electric pressure varies considerably. These difficulties have been thoroughly appreciated by workers in this direction. When an inter-connected set of circuits such as the three-phase system necessitates is used, it is found that, when the circuits are unequally loaded, there are great variations in the electric pressure, and the following tests were made at my request to illustrate this. The three circuits are called A, B, and C. The average electric pressure of the three branches at the terminals of the secondaries was maintained constant. A resistance tending to reduce the electric pressure at the lamps 5·3 per cent. was introduced into each branch. Both the primaries and the secondaries were connected as represented by the form of the letter Y. By varying the connections in different ways, some slight variations were possible; but this general fact remained—that when A is loaded and B and C are not loaded, the pressure of A is less than that of C, and that of C is less than that of B, and the difference may be 12 or 13 per cent.; and when A and B are loaded and C is not loaded, the pressure of A is less than that of B, and that of B is less than that of C. The variations here shown in the pressure of two equally loaded circuits is probably due to the armature reactions, and would be found in a two-phase system also; but the rest of the variation is due to the inter-connection of the three circuits, and is an obvious defect of the three-phase system. In order to get over this difficulty, the Œrlikon Company proposed to devote only one circuit to lighting purposes, which shall be maintained at constant pressure, using the other two circuits for power purposes, where constancy of pressure is not

so important. In some cases this would be a suitable method of working, but it was not considered satisfactory for the work at Niagara Falls. Having considered these points, the board of directors of the Cataract Construction Company, at their meeting in May, 1893, determined to reject the three-phase system, leaving the single-phase and the two-phase available. Professor Forbes.

Conditions.	Volts at Terminals of Lamp Mains.				Difference between Maximum and Minimum Volts.
	A.	B.	C.	Average.	
A, B, and C loaded (resistance out) ...	108·5	108·6	107·9	108·3	0·7
A, B, and C loaded (resistance in) ...	103·5	103·6	103·2	103·4	0·4
A loaded, B and C off	102·9	116·0	106·5	108·5	13·1
B loaded, A and C off	104·5	102·5	114·9	107·3	12·4
C loaded, A and B off	115·1	105·3	101·3	107·2	13·8
A, B, and C loaded (resistance in) ...	103·0	103·4	102·5	103·9	0·9
A and B loaded, C off	98·9	107·8	111·1	105·9	12·2
A and C loaded, B off	101·2	108·0	98·1	102·4	9·9
B and C loaded, A off	110·5	98·0	105·8	104·8	12·5
A, B, and C loaded (resistance in) ...	103·7	103·0	103·0	103·5	0·7

Frequency of Alternations.

I wish now to say some words on a question which has absorbed my most serious attention, especially since I have been acting for the Cataract Construction Company, and this relates to the frequency of the alternating current. In America a frequency has generally been adopted of 133 complete periods per second for lighting purposes. This was done with the object of reducing the cost of transformers, which were supplied to each separate house, and consequently were always of small size, and therefore

Professor Forbes.

expensive. The selection was also approved because American engineers working with alternating currents have generally put forward the view that parallel work is not desirable, and the fact that parallel working is more difficult at high frequency, which is one of the principal objections to high frequency, has not been seriously considered in America. In Europe the usual frequencies are from 70 to 100 periods per second, but a notable exception exists in the case of Messrs. Ganz & Co., of Buda-Pesth, who have adopted 42 periods per second. Some years ago I unwittingly did Messrs. Ganz the injustice to say that I thought it probable that they had adopted this frequency because it suited their particular type of machinery and speed of running. I have it in writing from them, and I am thoroughly convinced of the truth of it, that their reason in adopting the frequency of 42 periods per second was that it is the lowest frequency that is available with arc lights so as not to produce any serious flickering, and their desire was to lower the frequency as far as practicable in order to ensure parallel working. Of course it is a matter of common knowledge that parallel working is assisted by lowering the frequency.

With the large units which will be employed in connection with the Niagara Falls scheme, the total cost of the transformers is very much diminished; and thus the increased cost due to low frequency does not become so important a matter as it does when all the transformers are of small size — under 10 H.P., as is usual in electric lighting in America. Moreover, although with lower frequency the transformers must be increased in size, the increased cost is not in proportion to the lowering in frequency, because we can use a higher induction. Mr. Steinmetz has shown that the loss due to hysteresis varies as the induction raised to the power 1·6, and it is this loss which must be kept constant when we vary the frequency. I deduce from this law the fact that in any transformer, if the hysteresis loss is kept constant, its power of doing work varies in proportion to the frequency raised to the power 0·4 (but it is probably unwise to increase the induction so much as to saturate the iron). It follows that when we double the frequency we get out of the same transformer 132 units of work

Professor Forbes.

instead of 100.* If the frequency were quadrupled we should get 174 units instead of 100. I have been informed by Mr. William Stanley, jun., of Pittsfield, Mass., that without the use of theory, but simply working from his experience in manufacturing and testing transformers, he obtains almost identically the same law; and I have got independent practical testimony in the same direction from other manufacturers. It appears, then, that there can be no doubt that, in lowering the frequency, we are not proportionately increasing the cost; but at the same time it must be realised that the cost of transformers is to a certain extent increased by lowering the frequency. If the frequency be reduced to one-half, the cost is increased about 50 per cent. The lowest price which has been quoted for large transformers is 3·52 dollars per H.P., at a frequency of 42 periods per second. In halving the frequency the extra cost would therefore only be 1·76 dollars per H.P. It becomes, then, a matter of inquiry whether the benefits to be derived, by lowering the frequency in such a proportion, would compensate for the extra expenditure as indicated. I am thoroughly convinced that the gain is far in excess of this amount. I shall have occasion to discuss the superior efficiency of motors at low frequency; and in most types of motors I think it safe to say that, in passing from 42 periods to 21 periods, or varying the frequency in that proportion, we have a gain of at least 3 per cent. in the efficiency of the motors. Neglecting altogether the increased value of the motors from this cause, there is 3 per cent. more power at our disposal, which, at only 10 dollars per H.P. per annum, would amount to 30 cents per annum, or, capitalised at 5 per cent., represents an increased value of 6 dollars per H.P. of the plant, against which we have the increased cost of transformers—only 1·76 dollars. It appears, then, pretty certain that, from a purely economical consideration of the question, a lower frequency than any which has hitherto been adopted is advantageous.

With regard to the lowest limits at which we can work, since

* *Note, January 18th*, 1894.—Or rather, to get the same output, the coil is increased in the ratio 132 : 100, with increased hysteresis and less copper losses.

Professor Forbes.

our turbines have been designed to revolve at 250 revolutions per minute, a two-poled dynamo—if such could be satisfactorily constructed—would give a frequency of four and one-sixth periods per second, and none of the synchronising or polyphase motors employed at different factories could run at a higher speed than 250 revolutions. I am not sure that it is desirable that any motors should run at a higher speed than this, but in the few cases where this might be desirable the use of belts would be quite natural in order to give the higher speed. With a four-poled machine we would have a choice of speeds at 500 revolutions, 250 revolutions, and any sub-multiple of these speeds; for by increasing the number of poles in the motor the speed of the motor can be reduced as much as we please. With eight poles in the generator the maximum speed is 1,000 revolutions per minute, which is as high a speed as would generally be desired in connection with the factories supplied from Niagara Falls. This frequency is 16⅔ periods per second; and after considering the three points—namely, cost of transformers, speed of synchronising motors, and convenience in the design of the generators—I came to the conclusion that, so far as motive power was concerned, this is the frequency which would be most favourable for use at Niagara Falls.

Another indirect advantage of using a low frequency is of a very practical nature, and lies in the fact that the ordinary continuous-current dynamos, of any size that are made, may be used as synchronising motors by means of rings attached to the commutator bars in the manner which has been above described. This method of altering and working a direct-current motor so as to make it suitable for an alternating current of low frequency has long been known to electricians, but the attention of those who are not experts was forcibly directed to it by the machines shown by Mr. Schuckert and others at the Frankfort Exhibition in 1891. Owing to the above-mentioned fact, it would be possible for any manufacturer connected with our supply station to procure a motor of low frequency of any moderate power he might require at a day's notice, and this is not true of any other alternating motor, nor is it true of a higher frequency.

But the most obvious advantage of low frequency is the

improved efficiency and starting torque of motors. I have arrived at this conclusion from my own observations, but I find it confirmed collaterally. With synchronising motors, of course, the fact has long been thoroughly established that their performance is very much improved by using low frequencies. Again, those who have used the motors with rotating field of the two-phase or three-phase type have all been obliged to reduce the frequency of the current to get the best results. In this connection I am pleased to be able to show a diagram (Fig. 2), giving some tests which were made for me of a three-phase motor at 41 complete periods per second, and at 56 periods per second. Professor Forbes.

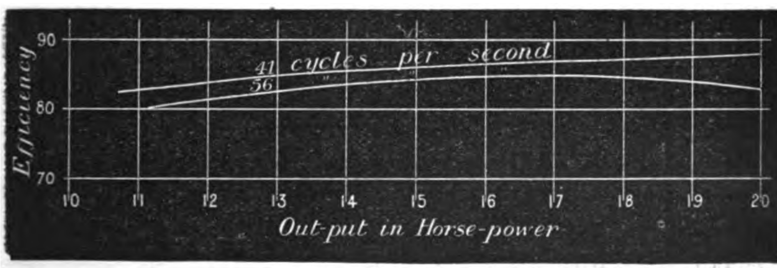


FIG. 2.

It will be noticed that at 56 periods the maximum efficiency is given when the output is 17 H.P. It is then 85 per cent.; and the efficiency at 41 periods per second at the same output is 87 per cent. But it will be noticed that the efficiency at the latter frequency goes on increasing until the output of the machine is 19½ H.P., when the efficiency rises to nearly 88 per cent. Thus, by lowering the frequency from 56 to 41 periods per second, not only had the output of the motor been increased 15 per cent., but the efficiency had also been increased 3 per cent. It is also found—which is a matter of the utmost importance—that in every self-starting alternating motor, whether multiphase or otherwise, the effort at starting is increased by lowering the frequency. (A theoretical view is considered at the end of the paper.)

Again, I find that the ordinary direct-current motor, with a laminated field, works extremely well with low frequency; even *without* lamination of the field it works, though not well. I have consulted nearly every electrician of experience in this direction

Professor Forbes.

whom I have met during the last year or two, either in Europe or America, on this point, and they generally agree with me that the facilities of working motors of whatever kind are very much greater with a low frequency. Last year, being aware that Professor Anthony had experimented considerably in the direction of series-wound motors with laminated field, I asked him his opinion, and he expressed himself as follows:—

"In reply to yours of August 30th, I have built two or "three continuous-current machines with laminated fields to "run with alternating currents, and have succeeded in running motors of about one-eighth H.P. directly from the Westinghouse converter, where the frequency is some 130 per "second. Such small machines run very nicely where the "alternations do not exceed 25 per second. In fact, so far as I "have tested, they seem to give an output fully equal to what "they will with continuous currents. Of course, with high "frequencies the self-induction of the field is against their "working, but at 8 per second I should say that large motors "could be run with perfect success."

I wish to repeat that, from what I have seen in the workshops of all advanced electricians in the last year or two, I am confident that, in the near future, single-phase alternating-current motors, self-starting on full load, will be largely used; and there is not the slightest doubt that all of these work far better with low frequencies. In fact, as Mr. Brush once said to me when I was discussing this matter with him, "Really, your "best plan would be to lower the frequency so much that you get "a direct current."

Whilst speaking of low frequency in relation to motors, I must say that I have much greater hopes of obtaining a good commutating device with a low frequency than with a high one; and I will also state that I have great hopes of important advantages coming to us from the invention of such a commutating appliance, which will enable us to furnish street railway companies, electro-metallurgical works, and other consumers with the direct current, without the use of any heavy revolving machinery at the transforming station.

I am not sure that it ought not to be said that the greatest

advantage of low frequency is in connection with the conductors used for transmission, and in the parts of the apparatus that require high insulation. When a high frequency is used there are certain difficulties which are well known. The first of these has been strongly urged by Lord Kelvin, namely, that when using large conductors, an alternating current of high frequency tends to confine itself to the outside of the conductor, thus increasing the total resistance. Attention was first generally directed to this subject by the Presidential Address of Professor Hughes to this Institution. A second difficulty is that, with high frequency, the impedance of the line due to the magnetic field formed between the go and return wires may amount to a very sensible quantity. Attention has been drawn to these matters by Mr. A. E. Kennelly, and his paper* on the subject should be carefully studied. Then, again, with high frequency there is a greater tendency to discharge from an electrified conductor into the air. This means that the insulating of a bare conductor is more difficult with a high than with a low frequency. Lodge and others have made this very manifest with exceptionally high frequencies, but the truth of the statement is well known by those who have experimented even with lower frequencies. Another trouble is that, whenever it is necessary to use solid insulation, a current of high frequency has a greater tendency to injure the insulation. Mr. Tesla has shown and explained so clearly the rapid deterioration of solid insulation by currents of enormously high frequency, that we cannot fail to see that advantage is gained by using currents of low frequency. Another very important fact is that, with low frequency, we are less troubled by the capacity of cables, and we have less loss by static charge, accompanied by heating of the insulation. Again, the serious troubles which have been encountered at Deptford and elsewhere, owing to abnormal rises of electric pressure in the mains above the pressure generated by the dynamos, due to the resonant effect produced by the capacity of the cable and the self-induction of the circuit, may be reduced as much as we please by sufficiently lowering the Professor Forbes.

* "Impedance," by A. E. Kennelly, American Institute of Electrical Engineers, April 18th, 1893.

Professor Forbes.

frequency. All these facts are the explanation of what has been thoroughly established in actual practice, namely, that there are difficulties on the line, when using high frequencies, which tend to loss of power and to destruction of the insulation, and that these difficulties are largely mitigated, if not entirely obviated, by reduction of the frequency. It must also be remembered that, if we reach a frequency as low as 16 periods per second, any induction in neighbouring telephone circuits is utterly inappreciable by the ear. Again, another advantage of low frequency is that there is a longer time when an arc formed has no current, and so becomes extinguished; hence, an arc is not maintained over so great a length with low as with high frequency. Finally, all eddy-currents diminish as the square of the frequency. Having now stated, as clearly as I can, what seemed to me the principal advantages of low frequency, I will place on the other side the disadvantages.

Besides the increased cost of transformers, there is one fact which is apt, at first sight, to impress one as almost fatal to the employment of very low frequency, but which further consideration shows to be of little moment in the case of Niagara Falls: this is, that a low frequency is not suitable for electric lighting directly. But it must be remembered that it is decidedly preferable to use a direct current for arc lamps, and, in fact, in the present position of the art in America it would be almost a necessity. Hence the natural method of arc lighting would be to use the alternating current, by means of a motor, to drive the well-known arc lighting machines. In the course of the work at Niagara, the first work in connection with arc lighting which will be set up is the lighting of Buffalo. At present this is done by means of steam engines, developing about 3,000 H.P., and driving arc lighting machines of the Brush, Thomson-Houston, and Wood types. There cannot be a doubt that, financially and practically, the best way of altering this station, so as to enable them to use the power from Niagara Falls, is to put in alternating motors in the place of the steam engines; and this will be the case in most of the towns which will be supplied with power from Niagara Falls.

It will be well at this point to say something about the frequency which is required to prevent arc and incandescent lamps from flickering. I have made a number of experiments on these points, and my conclusions are as follows:—A 16-candle-power 50-volt incandescent lamp shows a flickering almost up to 25 periods per second, at which frequency the flickering ceases when at its normal brightness; but if pushed to an excess of incandescence, the flickering is just perceptible up to 27 or 28 periods. I believe that a 100-volt 16-candle-power lamp shows a perceptible flicker up to 28 periods; but I may mention that this flicker is not nearly so serious or perceptible as that which frequently arises from the employment of certain types of engines, especially single-acting high-speed engines, when sufficient fly-wheel momentum is not provided. As a case in point, I would mention the lighting in the Holland House, one of the best hotels in New York, where, to an experienced eye, the flicker of the lights in the large dining-room from this cause is very objectionable. The thinner the filament, the more liability is there to such a flickering. I have lately examined the thick filament lamps to which the name of "Bernstein" lamps has been given, which consume 6 to 10 amperes at low voltage. It takes so long a time for the incandescence to die out when the current is stopped, that I have little doubt about their being able to work without any perceptible flickering at so low a frequency as even 16 periods per second. I have also made some experiments of a similar nature on arc lamps at low frequencies. At 37½ periods per second there was very bad flickering, and this was most noticeable when looking at a piece of white paper illuminated by the naked light, or when looking at an opal shade put on the lamp. At 40 periods it was still bad. Neither at this speed, nor at the previous one, was there any serious noise, but at 40 periods the noise could be perceived by putting a glass globe over the lamp, resting on the metal framework directly. At 41·7 periods there was just enough flickering to be objectionable; at 45 periods it was just possible to notice it on a printed page held close to the lamp, but it was not visible when reading at a distance of 10 feet. At 50 periods the only means of detecting

Professor Forbes.

Professor Forbes.

anything of the sort was by looking directly at the arc; nothing was seen when reading a book, either with the opal shade on or off. At this frequency the noise became much more perceptible, especially with a long arc about one-eighth of an inch. On reducing the length of the arc to one-sixteenth of an inch the noise was much less. In all these experiments the consumption of energy was at the rate of 26 volts and 14·2 amperes at the lamp terminals. The best cored carbons of Siemens & Halske were used.

An objection has been raised to the use of low frequency owing to the fact that a periodical twisting strain is given to the shaft of the dynamo, but this objection disappears almost entirely when we are dealing with a machine generating two phases.

It follows, therefore, that for arc and incandescent lighting at a very low frequency it becomes necessary to use alternating-current motor-generators, or else to use something of the character of a commutating machine to convert into continuous currents. Such machines as those exhibited by Schuckert at Frankfort in 1891 are useful up to a certain extent, but they are expensive, and comparatively inefficient, as their only function is to commutate the current—an operation which ought not to involve any significant loss. At the present moment a good commutator for the alternating current is not upon the market, but the matter is of such prime importance that I feel confident that much will be done in this direction. In fact, I have seen enough with my own eyes to have no fear about our being able to generate continuous current from the polyphase alternating current without serious loss, and with very inexpensive machinery. If, however, the object of our work was mainly, or even to a considerable extent, to provide the means of lighting towns by arc and incandescent lamps, I should have hesitated to recommend a reduction in frequency below 42 periods per second, such as is used by Ganz & Co., and which is operated so successfully at Rome and Tivoli, and at a very large number of other places on the Continent. The officers of the company and myself considered this matter most carefully, and, looking at the purposes for which our machinery is being set up, we felt sure that the proportion of electricity which would be used for

lighting purposes would not be large, and that we must look upon our whole plant as a *power* producing and distributing plant, and that our object must be to distribute *power* in the most efficient and economical manner. This being the case, we agreed that it was desirable to lower the frequency so far as the mechanical conditions of the problem would allow. The lowest frequency for which a design had been offered to us by manufacturers was that of a very beautiful design of machine, viz., 20 periods per second. This machine had admirable qualities, but was in some points not exactly adapted to the requirements of the situation, as developed by the selection of a special design for the turbine. Professor Forbes.

I have myself made several trials of designs at very low frequencies, even as low as eight and one-third periods per second, and for this frequency I prepared drawings of a machine which, though by no means perfect, shows a possibility of being worked into a sound machine of good mechanical construction; but further considerations led me entirely to modify the design of the machine, and eventually I arrived at the conclusion that, both from the point of view of design of the dynamo, and also for suitability of applying the current, 16 periods per second was probably as good as could be obtained. The manufacturers to whom the contract has been given were anxious to use a lower induction in the iron of the machine than that which I would have preferred, and this rendered the machine of 16 periods per second heavier than could be supported by the hydraulic piston which supports the whole weight of the turbine, vertical shaft, and revolving part of the dynamo. Consequently we have made a compromise, and are going to build our first three dynamos for a frequency of 25 periods per second. In concluding my remarks upon low frequency, I must again repeat that, from a purely practical and commercial point of view, one of the great advantages lies in the fact that, for any special purpose for which a motor is required, any ordinary direct-current motor may be altered so as to act as a synchronising alternating motor at a very small expenditure of time and money.

Professor Forbes.

ELECTRO-MOTIVE FORCE.

The question of selecting a suitable electric pressure for working to Buffalo, and also for local purposes, is of some importance. Generally speaking, it is desirable to use as high an electric pressure on the line as is consistent with safe and continuous working, as this effects a great saving in the amount of copper used on the line. In the first report, founded on insufficient data, which I wrote more than three years ago for the Cataract Construction Company, I recommended that 2,000 volts should be used in the neighbourhood, and that the pressure should be raised by means of transformers for the more distant transmission. But the greater portion of our work in the immediate future will have the character of distant transmission. In most of the tenders which were submitted to us the cost of transformers was almost as much as that of dynamos, in some it was more, so that the use of a step-up transformer for distant transmission meant almost double the cost of generating the current. I hold the view that a pressure of even 20,000 volts can be generated as safely in the dynamo machine itself as in the transformers, and that, if we used 20,000 volts for the local work as well as for Buffalo, we should not be incurring the additional expense and losses of a step-up transformer, while we should be saving enormously in the cost of copper. Unfortunately, American manufacturers have never supplied alternating-current dynamos at a higher pressure than 2,000 volts, and they are not practically acquainted with the experiences which have been gained in Europe at extra high pressures. Most of the manufacturers declared their inability positively, under any conditions, to go above 2,500 volts, although some of their engineers were willing to go as far as 5,000 volts. This, however, would have been no material gain to us; and the consequence is that, to meet the views of the manufacturers in our preliminary work—that is to say, in the construction of the first three dynamos for our power house—these will be only of such electrical pressure as they are accustomed to deal with. We shall therefore be using dynamos generating current at 2,000 volts, and employing step-up transformers for the extra high pressure. This may

Professor Forbes.

possibly render it desirable in our first work not to use the extra high pressure for local purposes. Besides the actual cost of the conductors, a very serious matter arises when we are dealing with the large currents due to comparatively low electric pressure. I refer to the large quantity of copper which has to be put into the conductors. Working at 2,000 volts, each circuit of each of our generators will give nearly 1,000 amperes, without considering the effects of retardation of phase, which will increase the current by quite a perceptible amount. So that, to put in the most economical section, it will require 3 square inches for each conductor, or 12 square inches of copper for each 5,000-H.P. dynamo, or 36 square inches section of copper for the 15,000-H.P. which is now being supplied. When we remember that, even with low frequency, the question of skin resistance comes into play when the conductors have a large diameter, it is obvious that we are introducing serious troubles, and if a subway were to be made to carry these conductors it would require to be of very great dimensions. For these reasons I am still anxious to see the extra high pressure used, even for the factories within a distance of a mile. The uniformity of the system would undoubtedly be of great benefit to us if we could generate the whole current, for all purposes, at the same extra high electric pressure, but, as I have stated, we were obliged to content ourselves with a somewhat less perfect arrangement than would be adopted if we were dealing with the utilisation of a waterfall in Europe.

I wish to lay stress on the importance which I have considered to lie in the fact of having a perfectly uniform system with interchangeable dynamos. Many engineers to whom I have talked, have suggested the use of special dynamos, at different pressures, for special purposes; but if we had a special dynamo for arc lighting, and another for incandescent lighting, a third for street railways, another for electro-deposition, and so on, the possibility of interchanging dynamos would disappear, and the whole system would be much more complicated to work.

I wish now to make a few remarks about the insulation which it has been customary to put upon dynamos and transformers which were to be used with high electric pressure. Many persons

Professor Forbes.' who have not given sufficient attention to the subject seem to be inclined to believe that there is something mysterious about the tendency of electricity in a dynamo or transformer to break through the insulation, and which prevents them from being subject to the ordinary laws of electricity. Thus, when building a dynamo for 2,000 volts, a thickness of insulation is given which would stand a test of more than 100,000 volts without breaking down, and it has been found from the ordinary methods of using the plant that, if something of this sort is not done, the insulation will break down. I wish to point out that the reason for this lies in the fact, not that the insulation breaks down with 2,000 volts, but that, in a 2,000-volt system as generally used, electromotive forces are occasionally generated amounting to 100,000 volts or more. These abnormal rises in electric pressure are chiefly due to the resonant effect, which has received so much attention of late years, and may be caused by the sudden breaking of the circuit of the dynamo. If these causes of excess be avoided, the electric pressure will never rise above the working pressure, and the insulation will never break down, even though its thickness be only little more than sufficient to stand a test at the working pressure. Dr. Fleming has shown us how to kill the resonant effect, and such a phenomenon never appears now at Deptford. This trouble may also be avoided by having as little capacity on the line as possible, especially when combined with low frequency. As to the cause of trouble mentioned above, I hold that it is a piece of culpable ignorance, ruinous to the machinery, if anyone should ever, on a large power circuit with alternating current, suddenly break the circuit while current is passing. This practice is quite unnecessary, and has given rise to a large proportion of the breakdowns of alternating-current machinery.

Parallel Working.

Engineers in America have had no experience in actual commercial conditions of parallel working with alternators. This is partly because the machines which have been made in that country do not work so well as some others in parallel. In the case of the transmission from Niagara Falls my opinion is that

Professor Forbes.

parallel working will give the best results. If this arrangement were not adopted it is obvious that, when the dynamos are loaded up as much as possible, they could never be all fully loaded. It is also quite obvious that, if our conductors are to be carried through subways, the space required becomes quite excessive unless we adopt parallel working. The complete success of this method of working between Tivoli and Rome left no doubt as to the feasibility, besides the desirability, of adopting parallel working. The reduction in frequency which we have made assists very considerably in this result, and it says much for the American manufacturer who has received the contract that, although parallel working with multiphase machines has not been adopted in the past, he is ready to guarantee the performance in this respect of the machines which are to be built for us.

The rules which govern the construction of machines which shall work well in parallel are not very clearly understood, each engineer having his own views, and differing from other engineers. The only fact which has been perfectly established in practice, is that, the lower the frequency, the more efficient and sure is the parallel working. It certainly depends to some extent upon the amount of self-induction and the amount of mechanical momentum in the machines. It is also certain that, if the self-induction of the machines is too high, they will not work well in parallel. Of all machines which have been constructed, those which work the best in parallel appear to be those of Ganz & Co., Mordey, and Elwell-Parker, but there are many others which do extremely well. It is, at present, not possible to state exactly the conditions which are necessary, but I may say that, generally, a machine with a stiff magnetic field, works better than one where the iron is far below magnetic saturation. In judging whether a machine of a particular type will or will not work in parallel, I think one must be generally guided largely by one's own personal experience in the matter, combined with a knowledge of the effects of self-induction, mechanical momentum, and magnetic saturation, as deduced from theoretical considerations. I do not think that anyone who knows anything of the working of the Tivoli-Rome plant would for a moment hesitate in saying that, on a large and

Professor Forbes.

important station like that at Niagara Falls, parallel working is essential; and this is the opinion of Professor Mengarini, who has so ably directed the works at Rome.

Motors.

Under this section it may be as well to discuss some of the different purposes for which the electric current will be required. With regard to arc and incandescent lighting, if the frequency be high enough, the current can be used directly for this purpose; although most people would prefer that, in the case of arc lighting, the current should be commutated or converted in some manner so as to give a continuous current. That this is unnecessary, however, is amply proved by the perfect success of the arc lighting by alternating currents in Rome, and many other large cities, especially those which have been established by Messrs. Ganz & Co. At the present time one of the largest applications of electricity in the United States is for street railways, which require a continuous current. Another similar purpose which we shall have to consider is the application to canal boats, since it is intended to work the Erie Canal by electricity. This canal starts from the Niagara River, and reaches the Hudson River at Albany, 350 miles distant. For these purposes some sort of commutator or motor transformer will be desirable. Some of the first work which will be done is the supply of much electricity at 150 volts for the production of aluminium. This also requires the continuous current, and similar means must be used for obtaining it. Among the large class of mills which will be established at Niagara Falls, one of the most important kind are wood-pulp mills, one of which is already working on our land, and will be the first to receive water power from our canal. This type of mill uses many thousands of horse-power, and is worked continuously day and night. In this feature it resembles, probably, a large number of the mills which will take advantage of the cheap power at Niagara Falls, and which will hardly ever require to be stopped or reversed. This is an important class of work in our case, because current can be supplied in such mills to synchronising alternators, as motors, whose

Professor Forbes.

efficiency is extremely high, and this may perhaps be done in some cases without transformers. We require also to consider the case of small motors for use in shops, and for elevators, cranes, and a large number of other purposes. In this class of work frequent stoppings and reversals are necessary. Hitherto direct-current motors have generally been employed for this latter purpose, but the rotating-phase induction motors are distinctly suitable, and when these have been fully developed they will probably be largely used for this purpose. The commutated current is a thing which is sure to be in common use before long, and although our arrangements at Niagara Falls are perfectly complete without a machine for this purpose, still its employment has been considered by us, and the probability of its future use has influenced our judgment in some points. There are so many purposes for which the direct current is most convenient, that people would generally accept it for transmission if it were capable of being economically transformed up and down to different pressures. The direct-current dynamo is really an alternating dynamo with a commutator placed upon it. If the commutator, instead of being placed there, be placed in the neighbourhood of the motor, we obtain most of the advantages both of the continuous and of the alternating current. This is one of the advantages of having two phases generated by the dynamo, because it is much easier to rectify a two-phase current than a single-phase current. In addition to the motors above specified, there are a large number of single-phase motors, which start with a powerful torque, which have been invented by different electricians, but which are not yet put upon the market. There are also machines with commutators, like a direct-current dynamo, with laminated field. These all become very efficient and useful when the frequency of alternation is sufficiently diminished.

Line Construction.

A great deal of attention has been given to the different methods by which current can be conveyed to the points of consumption, whether on our own property or at Buffalo, or even further. Naturally the pole line was first dealt with, in which the

Professor Forbes.

poles might be constructed of either wood or iron. This is the cheapest type of construction, and has some advantages, but we must consider the climatic conditions in the neighbourhood of Niagara Falls. We are subject there to severe thunderstorms, and troubles from lightning have already been serious in several parts of the United States in connection with electrical machinery. Snow and frost are very severe, and sleet forming upon the wires and insulators may cause a great amount of trouble. There are also at times very violent gales sweeping from over Lake Frie. All these difficulties can be counteracted to some extent, but it is nearly certain that, with overhead construction, occasions would arise when the continuity of operations would be interrupted, and this would be a very serious matter. The next system to be considered in order of cost would be underground cables, but I am strongly opposed to the adoption of these for any considerable length. It is true we are able to deal with their capacity so as to

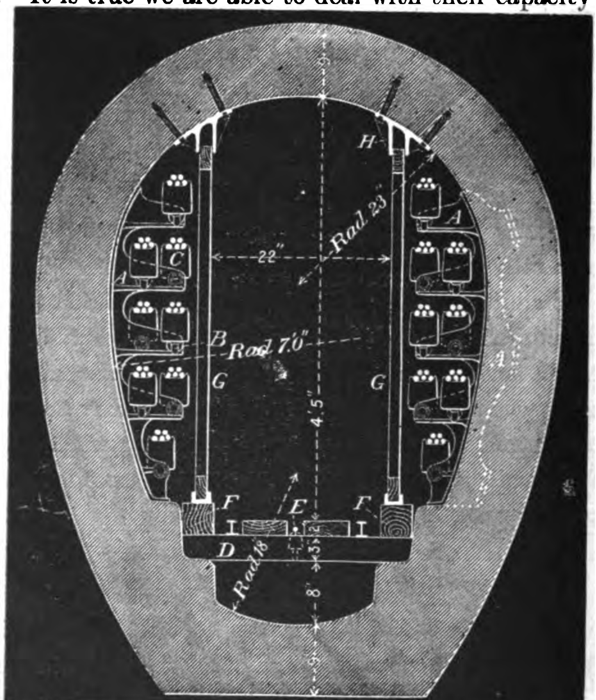


Fig. 3.—Electrical Subway—Cross Section.

Professor Forbes.

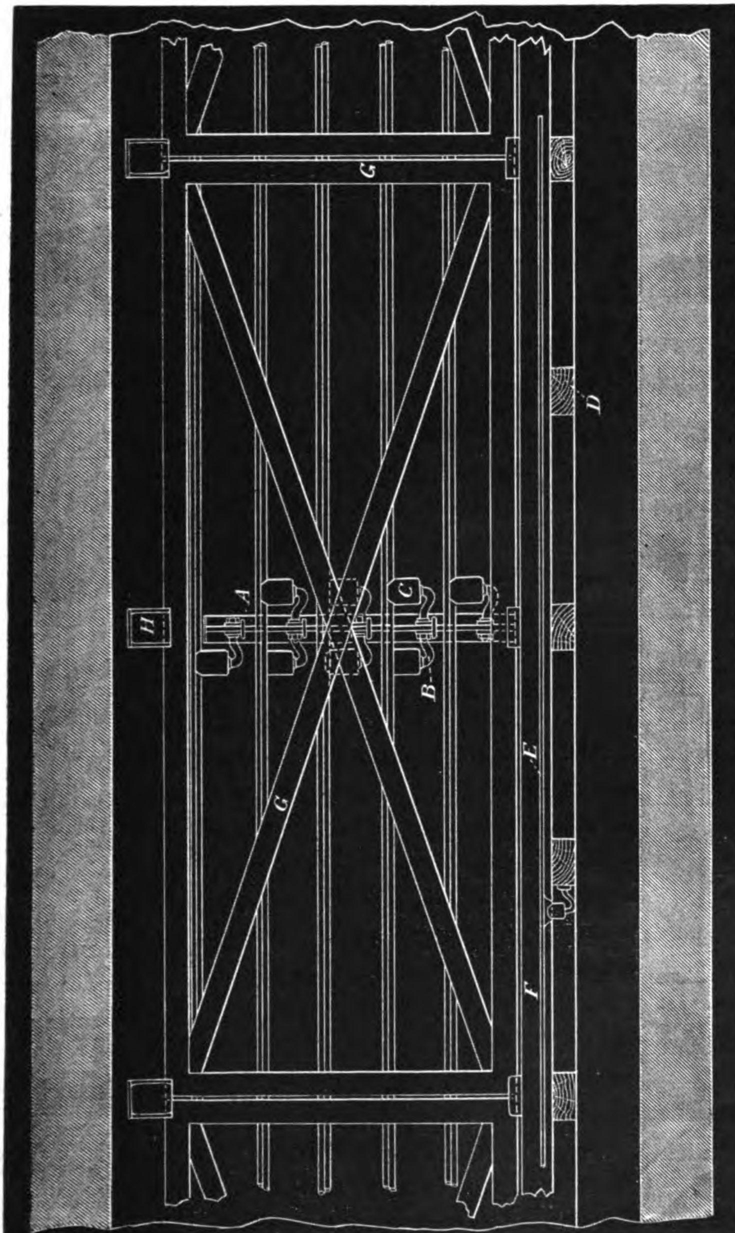


Fig. 4.—Electrical Subway—Longitudinal Section.

Professor Forbes.

reduce its injurious effects, but surely the best plan of all is to abolish the capacity itself as far as possible. The most satisfactory method of proceeding is to build a subway of sufficient size to permit of a person walking along it, with all the bare copper conductors in it, but this is a matter of considerable expense. I am glad, however, to be able to inform you that the officers of the company resolved last summer that a subway such as I have described should be constructed from the power house up to the Pittsburgh Reduction Company's works, where aluminium is to be produced, a distance of 2,500 feet. In accordance with their instructions, I prepared plans, which are now exhibited (Figs. 3, 4, and 5), which give space enough to carry the conductors at 20,000 volts for all the power that will be developed by the present tunnel, parallel working being adopted. The subway is built of concrete having a minimum thickness of 9 inches. The height inside is 5 feet 6 inches. Wooden beams are embedded in the concrete on both sides every 30 feet. When the concrete is set, these beams are removed, and iron castings (A, Figs. 3 and 4, and Fig. 5) for supporting the brackets that hold the insulators are put in their place, and these are then grouted in. Iron brackets, B, as shown in the drawings, are bolted to these castings, and the oil insulators, C, placed upon them. Copper wires are shown as being supported on these insulators, but we shall probably adopt copper strip. The bottom of the subway is always on an incline, and is of curved shape to drain off any water of condensation, but it is also proposed to force air through the subway so as to keep it dry. The 2,500 feet now being completed is all drained at the power house end by a boring into a water-bearing stratum. At the bottom of the subway the concrete is formed in two steps, the lower one of which supports sleepers, D, for a very light tramway, upon which a truck can run to carry supplies, and also to carry the inspector. At first this can be moved by a hand lever, but arrangements are made for eventually driving it by electricity, the conductor, E, being carried on the floor, between the stringers (placed on the ties or sleepers) which form a platform running the whole length of the subway. On the stringers, F, outside the rails are

Professor Forbes.

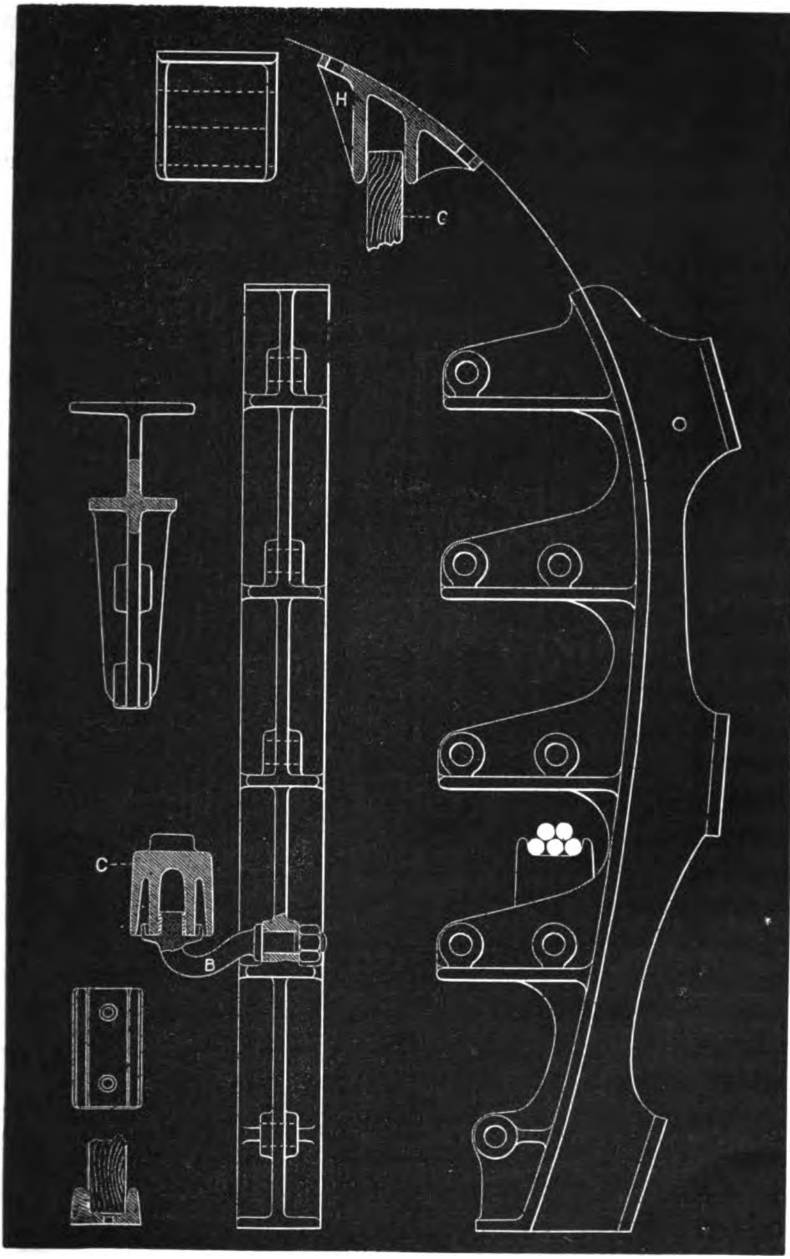


Fig. 5.—Castings for Brackets in Electrical Subway

Professor Forbes.

placed screens, G, separating, mechanically and electrically, the part of the subway where the conductors are from the part where the inspectors walk. This screen is formed in 10-feet lengths of wood supporting expanded metal, covered over with plaster up to a height of within 1 foot of the top, the upper part being left open in the form of a network, through which the inspector can look at the conductors and their supports. These screens are held by iron supports, H, fixed by expanding bolts into the concrete, and can easily be removed. The top of the arch is 3 feet below the surface of the ground, so that it will not be affected by frost. According to the last report, it appears that on the 21st of October 1,590 feet of this subway had been completed; the total length being put down at present is 2,500 feet. Each of the iron castings has a wire attached to it which passes through the concrete, and is soldered to a copper wire running outside the subway along its whole length, and connected to plates sunk in the water at any suitable points. At every 400 feet cross streets will be made, and at these points there is a manhole. Also at these points, on each side of the subway, four drain pipes, 3 inches in diameter, are let in, closed at their outer ends. When the subway is to be tapped for use on the side streets, or for intermediate points, wires can be laid through these pipes. Between Niagara Falls and Buffalo there is very little rock, but the part where the subway has now passed through has been chiefly in rock. This involved blasting out a channel larger than was required. In this case the part of the trench outside of the subway has been built up of stone. In the construction of this subway, American Portland cement has been used, and a very suitable sand from the neighbourhood, which contains its own gravel. The whole of this work has been done by Mr. Humbert, and up to the date of my last inspection every part was thoroughly and satisfactorily done. This work is of great importance as conveying to the minds of those who intend to use our power some idea of the desire to ensure continuous working. Fig. 6 gives a general view of the subway, and is taken from a photograph of the work.

With regard to the electric pressure that is to be used for

distant transmission, this will undoubtedly advance with experience. Professor Forbes.
Some manufacturers in Europe who tendered for the work

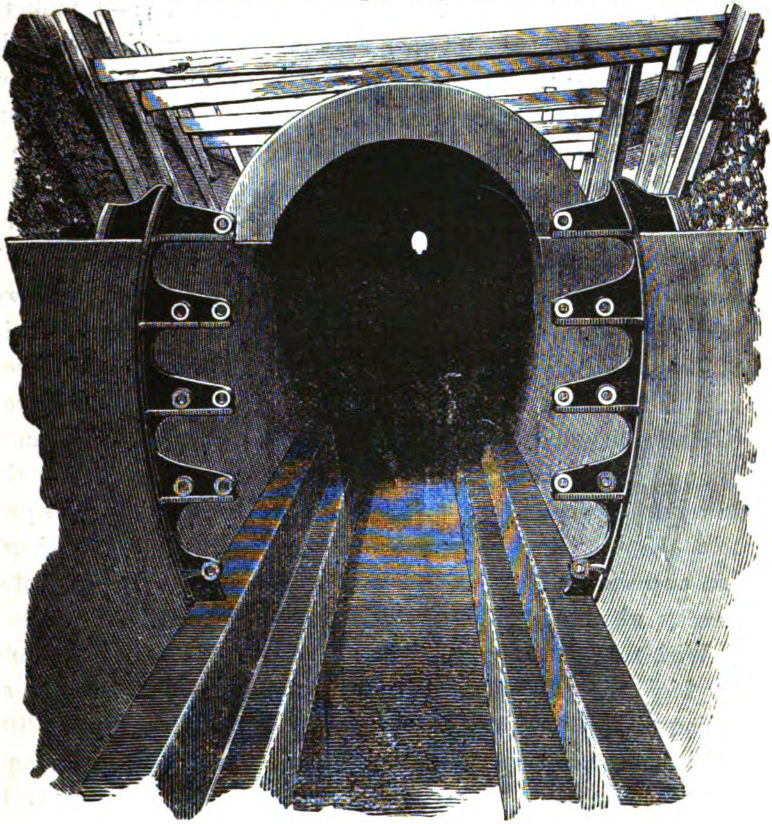


Fig. 6.—Electrical Subway during Construction.
(From a photograph of the actual work.)

proposed to adopt 25,000 volts, which is a step in the direction of Lord Kelvin's suggestion of 80,000 volts; but we have considered that at present 20,000 volts is not likely to be exceeded by us, and we may work at a lower pressure at the commencement. When it is remembered that the Deptford machines have one terminal connected with the earth, and are working satisfactorily at 10,000 volts, and when it is noticed that, in consequence, our work would be under exactly the same conditions as regards insulation when working at 20,000 volts, it is easy to see that we are not risking anything experimental in our first work.

Professor Forbes.

With regard to the size of conductors, I worked out the economical size at different current-densities for the whole distance. In doing this I took the following data:—I took the cost of copper at 12½ cents per lb., and the annual charge on this cost at 5 per cent. I then computed the power loss in the line, and the amount of power which was left available for delivery. I took the value of this power at the distant end of the line as being 15 dols. per H.P. This is something more than what it costs us to produce it at, but when the power available from our tunnel is nearly all consumed this quantity will have to be increased. It must also be remembered that I have not allowed for the increased size of the conductors required by the retardation of phase, which is an unknown quantity. Still, it will be seen that, from these considerations, we may be able to work economically at a slightly higher current-density than is obtained from this investigation. From this work it appears that the most economical density to work at is 350 amperes per square inch. If this density is used, the fall in volts between Niagara Falls and the northern boundary of Buffalo is only 3½ per cent.—a matter which makes regulation extremely easy.

With regard to the efficiency of the system, it is remarkable how high the efficiency of the dynamos comes out when we are dealing with the large units of 5,000 H.P. There can be little doubt that the efficiency, electrical and mechanical, of our dynamos may reach at least 98 per cent. Taking off 3½ per cent. for losses on the line, we would have 94½ per cent. delivered electrically at Buffalo, if no transformers were required to raise the electric pressure to the full 20,000 volts. In cases at Buffalo where the power consumed is very large, the motor can be constructed on the same principles as the dynamo; and if in this case it be ever possible to work at the full pressure without a transformer, it is obvious that the total efficiency of the system—that is, the power delivered by the motor to the shaft of the machinery, divided by the power delivered by the shaft of the turbines to the dynamo—will be certainly over 90 per cent. As a matter of fact, if we were to use a higher density of current, and were to use step-up transformers at

Niagara Falls, and step-down transformers at the northern boundary of Buffalo, and other step-down transformers in the town of Buffalo itself, and were to use motors of small power, and consequently less efficiency,—in this case the total efficiency of the plant might be reduced to 80 per cent., or even lower. Professor Forbes

I have given these figures, not as indicating precisely the lines on which we have determined to work at Buffalo, but because the present paper is intended to embrace the subject of the general distribution of power, and I thought it desirable to lay before you certain facts in this connection in a definite form.

Description of Machinery to be Used.

Under this heading I shall deal chiefly with the type of dynamo which has been finally decided upon. It will suffice to say of the turbines that they are each of 5,000 H.P., that they revolve at 250 revolutions per minute, and were designed by Messrs. Faesch & Piccard, of Geneva. All the principal parts of this machinery were constructed by the I. P. Morris Co., of Philadelphia, but the governors of the turbines are of Swiss manufacture, and part of the steel fittings were constructed in France.

With regard to the dynamo, the Cataract Construction Company at first invited many different manufacturers in Europe and America to submit plans. The number of these that were submitted altogether amounts to 24, some of the manufacturers having taken a great deal of trouble to submit a series of designs, in order to be able to meet different requirements. Many of these designs were extremely good; but it was determined, after estimating the increased cost of using the European designs, owing to the high tariff in America, and owing to transport, to have the machines manufactured in America. Among the designs there were none which fulfilled all the requirements of the case, and eventually the Cataract Construction Company decided to get out their own designs and to submit them to the American manufacturers for tender. I had been for some time previously engaged in working out a design, which I am confident none of the manufacturers who had sent in

Professor Forbes.

designs could say was in any way borrowed from their ideas. We had received suggestions of an external armature with internal revolving fields, and also of external fixed fields and internal revolving armature. We naturally had a preference for a dynamo with a fixed armature, because the coils can be more securely wound, and are not subjected to the mechanical stresses induced by centrifugal force. But all the designs with revolving fields which had been submitted to us contained a weak feature: the field coils were held in by pole faces secured by bolts or keys to the poles, and this seemed an element of weakness when we considered the enormous centrifugal forces to which the machinery would be subjected. At the same time, the turbine makers had insisted upon having a certain momentum, or fly-wheel effect, which was not given by the revolving part of any of the dynamos submitted to us, and it was found that, if any one of these had been accepted, it would have been necessary to add to the design a fly-wheel of large dimensions. Centrifugal force was one of the most important matters to be considered, because at 250 revolutions per minute this force assumes considerable magnitude in the large masses with which we had to deal. Moreover, in all the designs which had been submitted, the magnetic pull between the poles and the iron of the armature assisted the centrifugal force. The principal feature of the design upon which I was working, consisted in having the armature fixed, and inside the machines, with the fields revolving outside; the fields being formed of a ring of iron with the poles projecting radially inwards. One advantage is that we are able to get the full fly-wheel effect that was required by the turbine makers. This requirement was that in the revolving part the sum of the weight in pounds of each part, multiplied by the square of its velocity in feet per second, should equal 1,100,000,000. The design also gives an extremely good mechanical construction for the revolving parts. The iron ring which forms the yoke of the fields serves as a support to hold in the pole-pieces and the exciting coils, and no part is held in against centrifugal force by bolts or keys. Moreover, the magnetic pull between the fields and the armature acts in opposition to, and does not assist, the centrifugal force. Inside

Professor Forbes.

the fixed armature is a large space available for the entrance of workmen to attend to the bearings, and to reach any part of the armature. It is obviously possible to insulate the armature coils for any electric pressure. With a large machine of this kind, the space occupied by insulation has not the same importance in reducing the output of the machine as is the case with machines constructed in the past. In fact, the armature coils can be wound with the same insulating properties as a transformer, and hence the necessity for using a step-up transformer can be avoided.

Starting with this general principle, I first got out designs of a machine at 33 periods per second, in order that I might compare the general appearance of such a machine with those of other types. The next point was to design a machine of as low a frequency as was possible under the limiting conditions as to weight. The revolving parts of the turbine and dynamo, and the shaft connecting them, are supported by a hydraulic piston, and it was the desire of the Cataract Construction Company not to use a thrust bearing of any kind to support the weight, although there is a thrust bearing at the top of the shaft, which simply acts to retain the apparatus in a fixed position in a vertical direction. Owing to this decision, it was necessary to limit the weight of the revolving parts of the dynamo to 80,000 lbs. In getting out the design for this machine it was desirable to select such a form of winding as past experience led one to believe would be most efficient for parallel working, and I judge from past experience that this is best attained by having the number of coils per pole very small. The 33-period machine which is shown in the drawings (Fig. 7) has only been sketched out with a view of getting at the general dimensions of the machine. It is not suitable for our special purpose, because it is essential that, without taking the dynamo altogether to pieces, we should be able to lift up through the centre of the dynamo any parts of the turbine shaft which may require repair. A small view of the machine, *a*, shows the means of gaining access, by steps, to the interior of the fixed armature.

It will be seen that, from the necessities of the case at Niagara

Professor Forbes.

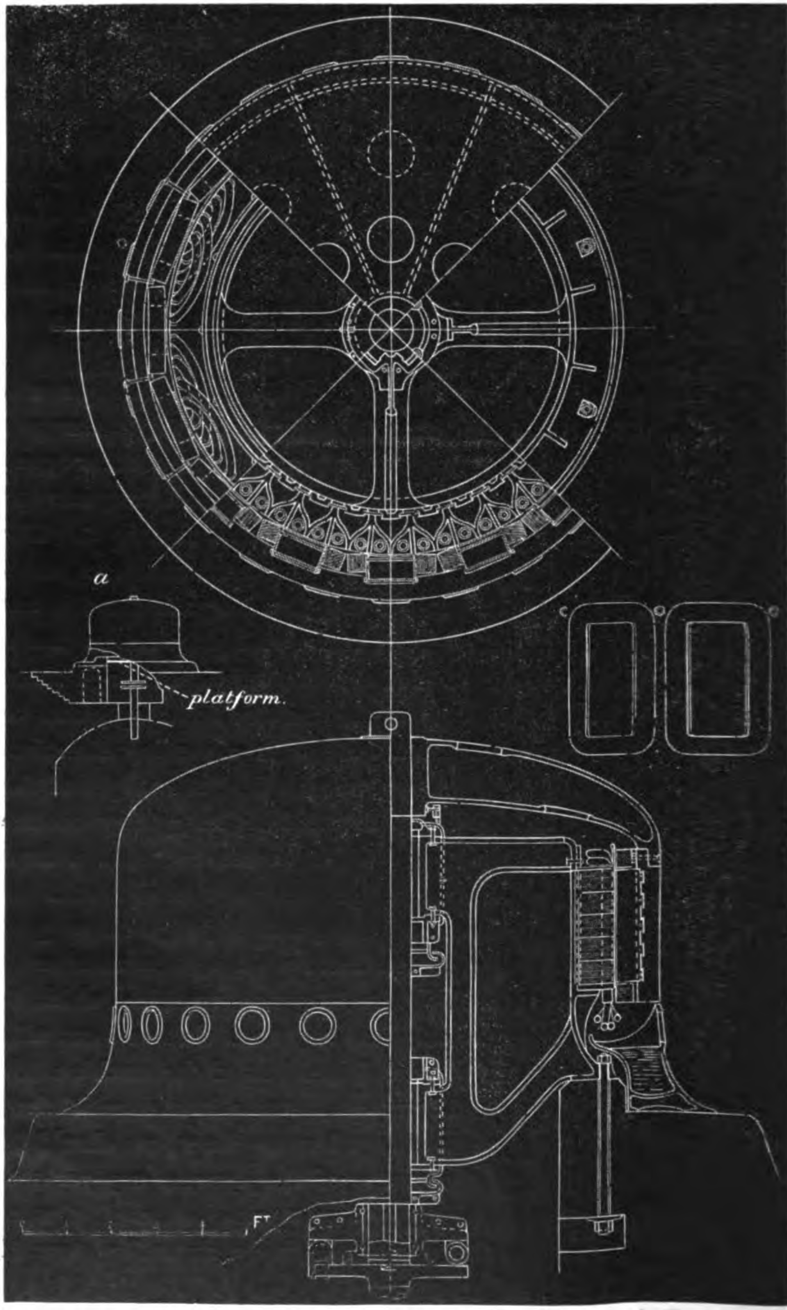


Fig. 7.—5,000-H.P. Alternator.
2 phases, 83·3 periods, 20,000 volts, 250 revolutions

Falls, the dynamo which was required differed in some respects from what would be necessary in many other cases, but it is equally obvious that, in any case of so large a transmission of power, the conditions must be thoroughly considered beforehand, and the dynamo specially designed for the purpose. At the meeting of the board of directors of the company, in May, 1893, I was instructed, with Dr. Sellers, to get out plans for an alternator of the type which I have described. Professor Forbes.

I am not able to lay before you now plans of the machine as it has been finally adjusted by conference between ourselves and the manufacturers. In the plan which we prepared, the frequency was $16\frac{2}{3}$ periods per second, there being eight poles. The armature coils were so wound that they might be connected to give either 2,500, 5,000, 10,000, or 20,000 volts, and the coils were limited in number so as to give the best assurance of good parallel working. For various reasons we decided eventually to raise the frequency to 25, and to lower the volts to 2,000, without the means of connecting the coils to give a higher pressure; and instead of winding the conductors on the armature in a limited nnmber of coils, to adopt the methods more commonly used in some of the large types of generators. But since in any future work which is done, the dynamo will be required to be modified for the special purpose, a description of the machine of which I am able to show you the drawings is sufficiently representative of the type of machine for our purpose.

General Description of a 5,000-H.P. Alternator.

On the bed-plate a vertical cylinder, A, Figs. 8 and 9 (Plate 1), is bolted, with projections to support the fixed armature and the bearings of the revolving part. The armature coils, E and F, are wound independently, and can be removed and changed. They are fixed in slots, C, in the fixed armature. They are encased in an oil-tight casing through which oil can be circulated. The field magnet is external to the armature, and has the poles pointing radially inwards. It consists preferably of forged steel, supported by a spider, D, with eight arms, which may be of steel (shown as a bronze casting in the drawings) with

Professor Forbes.

a covering of thin sheet metal, on which cups are provided for forcing air into the interior of the machine. The pole-pieces are bolted on to the steel rim, B. The field coils are of copper strip, two coils being wound upon each pole, with a space between them for air circulation. The exciting current is applied by rings of tempered copper on the spider, having fixed brushes rubbing on them, shown at G. The hub of the spider is firmly fixed to the upper end of the shaft. The spider supports the heavy rim by 16 studs and nuts. The shaft is supported by two bearings, each of which has four radial arms, H, which are bolted to four corresponding projections on the cast-iron cylinder. This cast-iron cylinder is bolted to the bed-plate at I, and adjusted thereon by wedges. Besides having on it the projections for carrying the bearings on its inner side, it has on its outer periphery a series of vertical ribs, K, against which the stampings, or sheets of iron, forming the armature rest. It also supports the lower end plate, L, on which the armature is built up, and the armature is keyed to it by a single key. The armature is wound drum fashion—that is, all on the outside. This enables the coils to be wound independently and afterwards laid in their place, and also to be easily replaced in case of accident. In order to do this satisfactorily, the slots for each coil in the iron are cut, not radially, but parallel to each other, such as the two marked C in Fig. 9. Each coil is encased in insulating material, which forms a tube through which oil may flow as in a transformer, but may be forced, in which case its circulation is maintained by a pump. Round the base of the machine there are two oil pipes, A and B, one being the inlet, the other the outlet. These are seen in Fig. 10 (Plate 1), which shows the improved oiling arrangements, which were worked out by my chief draughtsman at Niagara Falls, Mr. Baumann. The inlet pipe is connected with a reservoir of oil in the power house. The outlet pipe leads by another pipe to cooling arrangements in running water, from which the oil is pumped to the reservoir. From the inlet pipe 16 brass tubes, C, are led to the junction boxes at the bottom of the 16 coils; the 16 other brass tubes, D, are led from the junction boxes at the top of the coils, through

Professor Forbes.

the interior of the fixed armature, and bent over the foundation plate, and so connected to the outlet pipe which surrounds the bed-plate. The outlet pipe is broken at E, a cup being placed on the lower part to catch the oil as it issues from the upper part, so that the rate of flow of the oil may be seen. The bed-plate is a single iron casting, and, in order to enable it to be transported over the railways, it was necessary to limit its diameter as shown in Fig. 10. The armature is built up of thin sheet iron, with ventilating spaces, as shown on the drawings. The bolts which hold the armature together are eight in number, to be made of nickel steel, a metal which has the great advantage of being non-magnetic, and of very high electrical resistance and great mechanical strength. The amount of nickel is 25 per cent.

There are 16 armature coils, eight being of one kind, called "short" coils, and eight of another kind, called "long" coils, the length of wire on each coil being the same. The short coils are bent over on the top and bottom end plates, and the long coils are wound in one plane and enclose the short coils. This is clearly shown in the drawings, where the "short" coils are marked E and the "long" F. I find that if we are to have a very stiff field, with first-class quality of iron in the fields, each coil would consist of 72 turns of pure copper wire, No. 0 of the Brown & Sharpe gauge, which gives a very low density of current and reduces the heating. In fact, it has been my view that, in designing the machines for so great and permanent a work, every effort should be made to reduce the rise in temperature, even to far below what has ever been done in the past. The unequal expansions and contractions of materials in a machine of this kind are very injurious to its permanence, and affect the insulating material seriously. The layers of wire in the coils are separated by mica. Each coil when wound has strips of insulating material laid spirally round it, so that oil may circulate freely in the intervals, and the whole is encased in a casing of strong insulating material. The material which I prefer for this purpose is certainly Woodite, an eighth of an inch of which will not break down with 30,000 volts, and which is not acted upon by oil even at high temperatures. I have a record of

Professor Forbes

tests with oil which are conclusive on this point. It is quite the best material I have seen for the purpose, though costly, and, being a secret process, it may be difficult to ensure uniformity.

The frames of the bearings are of cast iron, with four radial arms, which rest upon four projections cast on the vertical cylinder, and are bolted to them, as already stated. By this means, when the field magnet with the shaft has been lifted out of place, the bearings can be twisted through an angle of 45 degrees in a horizontal plane and then raised out of place, leaving a space 5 feet in diameter through which portions of the turbine shaft can be raised for repair. The bearings are oiled at the centre by oil forced under pressure through a pipe. The oil is then distributed over the bearings by spiral grooves. A spiral groove is also cut in the hub of the frame, with a pipe at each end to admit of water circulation to cool the bushing. At two opposite sides of each bearing the bushing is made thin, and a rod of bismuth is soldered thereto; so that if the bushing is heated a thermo-electric current shall be created which, by means of a relay, can ring a bell in the power house. When this occurs, water can be immediately admitted to cool the bearings, and the attention of the workmen is drawn to the necessity of an examination.

One of the parts which required most consideration was the material of which the spider supporting the field magnets should be constructed. The best plan is to make it of steel (although, as pointed out before, it is shown in the drawings as a bronze casting), for the sake of lightness, and to cover it with a copper covering, which might, I think, be spun; or, perhaps better, electro-deposited nickel might be used.

Between the pole-pieces the space is filled up with a screen or plate of metal so as to direct the air ventilation only upon the parts which most require cooling.

It will be obvious from this design, considering that the speed of revolution is 250 per minute, that great care has to be devoted to the balancing of the revolving parts; everything has to be calculated not only for a speed of 250 revolutions, but for double that amount, which is the maximum speed at which

the turbines could possibly run—at which speed they might run through a breakdown of the governor, although this is an accident almost impossible to occur. Each of the revolving parts will of course be balanced individually, and I have suggested a plan for the final balancing which seems likely to be effective. A temporary bushing would be put in the bearings, of india-rubber lined by a thin metal tube. The dynamo would then be rotated slowly and the balance adjusted in the usual way. The speed of revolution would be gradually increased—a new adjustment being made at each speed—until a speed of 500 revolutions a minute is attained, and when an adjustment has been made at this speed it is pretty sure that the balance at 250 revolutions per minute will be very perfect, and the mechanical friction reduced to a minimum. Professor Forbes.

As stated before, the oiling arrangements for the coils have been introduced not only to ensure higher insulation and to preserve the insulating material, but also to lower the temperature as much as is possible, because every step we take in the reduction of temperature is an advance. These oiling arrangements may perhaps be adopted in the future, but at present we are not making use of them in the machines which are in process of construction. When we have had experience with these we may in future adopt the oiling arrangements; and as there is likely to be a great development in the direction of electric transmission of power in connection with the utilisation of water power in many parts of the world, it has seemed well to me to put before you these details, so that they may be considered in other cases that may arise.

At a meeting of the board of directors in New York lately it was resolved, on the recommendation of the consulting engineers of the company, that the contract for two or three alternators, each of 5,000 H.P., should be assigned to the Westinghouse Electric and Manufacturing Company, of Pittsburgh. I would wish to state how much we owe to Mr. Westinghouse, and to his chief engineer, Mr. Schmid, for the zeal with which they have taken this matter up, and their desire to meet our views and to secure for us machines of which they felt they could guarantee the satisfactory performance.

Professor Forbes.

Before leaving the subject of the dynamo, I would wish to point out that it has been designed for special circumstances in connection with the Cataract Construction Company's work. If a dynamo of the same type were being constructed for another place, it is certain that modifications would have to be introduced. I particularly draw attention to the fact that some trouble in getting out a good mechanical design arose from the necessity which existed of providing a clear space of about 5 feet diameter in the middle of the machine, without taking the whole machine to pieces; the object of this being to enable us to lift up portions of the turbine shaft which it might be required to put into repair. In any case where the long shaft existing in our case is not required, the design of the dynamo is much simplified, and would more nearly approximate to the first design of which a drawing is shown (Fig. 7), but which is arranged for 33 periods a second.

If the present paper were intended to relate solely to the subject of the utilisation of power at Niagara Falls, I would be content with describing what has actually been done, but I foresee that there is going to be a great development in utilisation of water power, and its electrical transmission. I am therefore inclined to say a few words on some other details which we have been carefully considering, but about which no definite decision has been arrived at.

Before doing this, I would direct your attention to the drawing representing a plan of our power house (Fig. 11), in which you will perceive the inlet passages, or head-races, A, from the great canal, B, which draws its supply of water from the upper river. From these inlet passages the iron pipes, or flumes, C, pass vertically downwards to the bottom of the great wheel-pit, which is a slot cut in the ground to a depth of nearly 200 feet, at present large enough to contain four turbines in line, but which can be extended to a much greater length, the whole capacity of our tunnel being 100,000 H.P. The drawing shows circles, D, which indicate the positions of the turbines and dynamos above them; and the positions of hatchways, F, by which materials can be raised or

Professor Forbes.

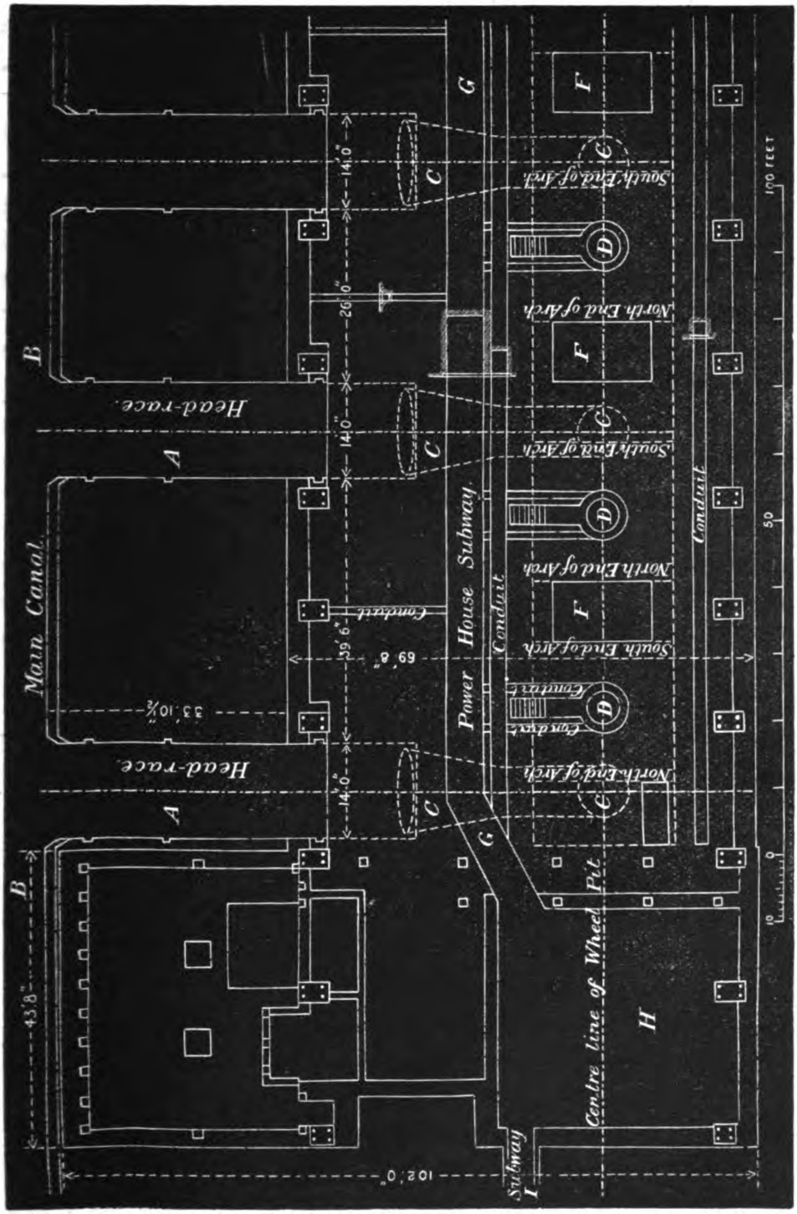


FIG. 11.—Plan of Power House.

Professor Forbes.

lowered, are also shown. Between the inlet channels and the wheel-pit the flumes are bent downwards, and thus leave a V-shaped space, which I have appropriated to make a subway, G, running along the whole length of the power house, to carry the high-pressure conductors. It will of course be understood that the slot forming the wheel-pit is arched over at the top, as indicated, to form the floor of the power house, upon which the dynamos rest. It will be noticed that, at the north end of the power house, there is a large square chamber, H, in which I propose that all the measuring instruments and other apparatus under the control of the chief electrician shall be assembled. Underneath this chamber there is a cellar, in direct communication, first, with the subway which I have described as existing in the power house, and, second, with the subway, I, which leads outwards from the power house, at present as far as the Pittsburgh Reduction Company's works, but which may possibly eventually lead to Buffalo. In this cellar all the high-pressure wires, the transformers, the artificial load, and other high-pressure machinery, will be placed. It will be noticed that other spaces are left in the floor, forming conduits along which the conductors can be carried. With these arrangements there can be no possibility of danger to any person in the power house; and if any wires are to be found laid along the walls of the power house or elsewhere, I wish it to be a maxim that we shall be able to place upon these wires a card marked "NO DANGER," so that there will be no possibility of danger to any person touching anything in the power house.

With regard to the exciting current, the best plan available at this moment is to use one of the machines which are generally known as the Schuckert machines, for converting the alternating into a continuous current, transformers being inserted in order to lower the pressure. In order that the exciting current may increase with the load, it would be well to make these transformers of special construction, each having two primaries and one secondary. One of the primary coils would be in series with the main circuit, and the other in shunt. I would furthermore make these transformers sufficiently large to deal with all the

Professor Forbes.

dynamos which are in the central station, and I would subdivide the secondary coil into sections, to enable us to cut out a section of the transformer when we cut out one of the dynamos. When we wish to cut out a dynamo, a switch would be worked which would at the same time short-circuit the field coils of that dynamo and also cut out one section of the secondary of the transformer which is supplying the exciting current to all the dynamos. This plan allows the fields of all the alternators to be put in series—a desirable arrangement for parallel working. A resistance may of course be put in circuit with the fields of the alternators for regulation.

I presume it will be taken for granted that, in any large work of this sort, the primary circuit should never be broken when in action. For my part, I hold that this should be the case even in smaller stations.

An important feature for putting the dynamos in parallel, and for removing a dynamo, is an artificial load. It is desirable that this artificial load should consist partly of a resistance and partly of self-induction. It is only by this means that the dynamo which is going to be put in circuit can be brought to exactly the same condition as those which are working, both as regards volts and amperes. It may be well to describe the operations which take place when a dynamo is switched in parallel with the others. First, connection is made beween the armature and the artificial load; then the exciting switch is turned so that an extra section of the transformer is put into play, and the short-circuit on the field coils of the dynamo is broken. The dynamo being excited, the turbine is then started, or this may be done at first. The artificial load is then adjusted until the dynamo is giving the same volts and amperes as the others. A synchroniser is then placed between the artificial load and the external circuit, and so soon as synchronism is attained a switch is closed which connects the artificial load with the external circuit. Resistance is then put into the artificial load until there is very little current going through it, when it is switched out, and the dynamos are all working in parallel. To cut out a dynamo from the circuit the operations are performed in the

Professor Forbes.

opposite order. The artificial load with high resistance is put in connection with the external circuit; this resistance is gradually diminished until it indicates the amount of work that is being performed by one dynamo; the connection between the artificial load and the main circuit is then broken, leaving the dynamo (which is being switched out) feeding the artificial load. The resistance of the latter may then be increased, and the exciting switch actuated so as to short-circuit the fields of the dynamo, and to remove one section from the secondary of the exciting transformer. The supply of water to the turbine may then be shut off.

If machinery is worked, even at 20,000 volts, in the manner I have described, there is no possibility of injury from any great rise of electrical pressure, unless the external circuit be by any means accidentally broken. To provide against this sort of trouble I would have wires coming from the external circuit where it enters the power house, connected through a large resistance, or through the primary of a transformer the secondary of which contains a resistance. In circuit with it I would have a break, consisting of two carbon points at a distance of about half an inch apart if we were dealing with 20,000 volts, so that an arc could not be formed unless the pressure rose above the normal value. Under these circumstances, so soon as any resonant effect due to the breaking of the circuit, or due to any other cause, raises the electric pressure above the normal, an arc is established across the carbon points, and so a load is put on which removes the cause of the extra high pressure. This is the only automatic means which I have been hitherto able to think of, which is sufficiently rapid in its action to overcome any possibility of injury to the dynamo or transformers.

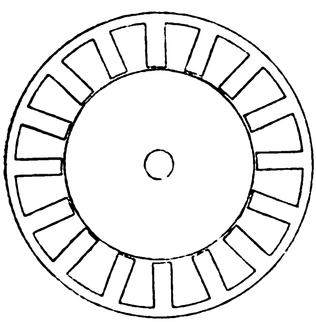
I have attempted in the course of this paper to give you some idea of the work which has been actually done or decided upon at Niagara Falls, and also to show you the views to which I have been led, by any special experiences which I may have had, as to the general ideas which ought to guide us in the construction of plant in the future for transmitting power to a distance electrically.

Professor Forbes.

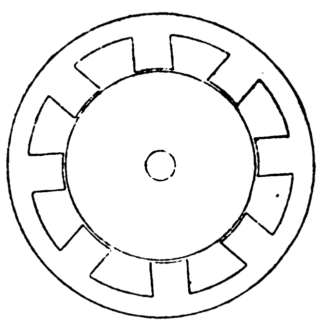
In describing the different plans which are available, I have avoided mentioning the names of those numerous electrical engineers and manufacturers who, by their inventions, researches, or applications, have advanced the art; or discussing their claims to priority; but I cannot conclude this paper without mentioning the names of some of those who, in one way or another, have made great steps in the applications of alternating currents to power purposes. I would particularly mention the names of Messrs. Ganz & Co., Mr. Schuckert, the Allgemeine Electricitäts Gesellschaft, of Berlin, the Œrlikon Fabrik, and Messrs. Brown, Boveri, & Co; also Mr. Eickemeyer, Mr. Ferranti, Professor Ferraris, Professor Fleming, Dr. J. Hopkinson, Messrs. Hutin & Leblanc, Mr. Rankin Kennedy, Professor Mengarini, Mr. Mordey, Mr. Tesla, Professor Elihu Thomson, and Mr. Henry Wilde. I feel that all of us owe a great deal to their work.

In conclusion, I wish to draw attention to the figures *a* and *b*, below, which show the relative merits of high and low frequency with polyphase motors.

a



b



Everything is identical in the two figures, except that, the frequency of one (*a*) being double of the other (*b*), *a* has 16 poles, *b* has 8. The armature is identical in both, and revolves at the same speed and does the same work; and the field revolves at the same rate in both, and the induction and current-density are the same. The differences are that *a* has more copper and less iron than *b*. The comparison of efficiency depends on the depth of both. As an example, assume that *a* has 50 per cent. more

Professor Forbes.

copper in the fields than b, the ampere-turns per pole being necessarily the same in both, and that b has 50 per cent. more iron than a, and that the hysteresis loss in b ($= H_b$) is equal to the resistance loss in its copper coils ($= C_b$). The values for a are—

$$H_a = 2 \times \frac{2}{3} \times H_b;$$

$$C_a = \frac{3}{2} C_b;$$

$$H_a + C_a = \left(\frac{4}{3} + \frac{3}{2}\right) \frac{H_b + C_b}{2} = 1{\cdot}42 \times (H_b + C_b).$$

Thus the total losses in the field of higher frequency (neglecting eddy-currents) are 42 per cent. more than in the field of lower frequency.

The PRESIDENT: Gentlemen,—As it would not be possible at this late hour to commence the discussion on this important and interesting paper, we must postpone it until our next meeting; and I will content myself, therefore, now by only asking you to accord to Professor George Forbes your hearty thanks for the care, the labour, and the zeal with which he has brought this subject before you.

The resolution was carried by acclamation.

The meeting then adjourned.

The Two Hundred and Fifty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 23rd, 1893—Mr. W. H. PREECE, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting of November 9th, 1893, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The PRESIDENT: It always is an extremely painful duty on the part of the President of this meeting to have to refer to gaps that are made in our ranks. Unfortunately, they happen rather frequently, and with the growth of the Institution they naturally become more numerous year by year. We do not refer to every loss that we may sustain from death, but when we lose a prominent member,—one who has been in the habit of taking part in our discussions,—one who was a friend of many of us,—I think we should be doing wrong if we were to part without making reference to the work he has done. We have the extreme regret to record on our black list the loss of one whom we all loved very much indeed, and that was Mr. Anthony Reckenzaun. As an electrician he held a very high position. We all listened with a great deal of attention to the opinions he expressed, and we are all sorry to lose from our midst one who took such an active part in our proceedings. In expressing our great grief at his loss, I feel confident that I shall receive the support of the whole of the meeting when I ask you to allow the Secretary to express, in the name of the Institution, our condolence with his widow and family at the severe disaster which has occurred to them. Perhaps, as a matter of form, somebody will be kind enough to propose that.

Mr. GISBERT KAPP: I propose that a vote of condolence be officially communicated to Mrs. Reckenzaun, expressing our sympathy with her in the loss of her husband.

The motion was seconded by Professor George Forbes, and unanimously carried.

Mr. W. H. Patchell and Mr. W. H. Blakeney were appointed scrutineers of the ballot.

The President: We are now, gentlemen, about to commence what I hope and expect will prove to be a very valuable discussion on Professor George Forbes's paper on "The Electrical Trans-"mission of Power from Niagara Falls," and I will ask Professor Fleming to be kind enough to open it.

Dr. Fleming.

Dr. J. A. Fleming: In opening the discussion on this paper which Professor Forbes has presented to us, it is hardly possible to do anything else than to congratulate the Institution on receiving such a very valuable contribution to the pages of our Journal. It is not always that engineers can be found who are able or willing to take us into their confidence during the constructive stage of their work, and to enable us to hear from them not only what they have done, but what they are proposing to do, and to have, as it were, the workings of their mind exhibited to us during the progress of the undertaking with which they are connected; but we have had that advantage in the present paper, and that, I think, contributes in a very large degree to make it such a suggestive and valuable one.

I think we must also congratulate, not only this Institution, but the Cataract Construction Company on having engineers as their advisers who have been willing to take the enormous trouble that Professor Forbes and his colleagues have taken in, as it were, going round the world and examining most carefully, deliberately, and thoroughly all that has been done in large schemes of power-transmission before making their choice, and before committing themselves to any system. Probably it would have been a much more easy thing for them to have elaborated out of their own minds something exceedingly new, some very original system; but they adopted the exceedingly prudent and wise course of, first of all, thoroughly sifting all that had already been done, before committing themselves to important decisions.

There is very much in the paper to stimulate and suggest discussion, but I shall only venture to trouble you with remarks on two points, leaving all the rest to be discussed by those who

are perhaps more familiar with some of the points than I am myself. The two points on which I shall touch are, in the first place, the difficulties which may be expected to appear in all cases where electrical transmission of power is conducted over long lines, which effects and difficulties arise from the presence of capacity and self-induction in the line, and in what may be called the "receiving instruments" at the other end. These difficulties are very real, and they have had to be overcome in cases which are now working, and they are to be by no means set aside as light matters. It does not do to assume that we can take for granted that the effects of capacity and self-induction in these long lines can be neglected because they do not make themselves disagreeably apparent when we are dealing with shorter lines of a mile or two. They are not merely hypothetical things which are discussed by professors, and which may therefore be very safely neglected when you come to practice, but they are exceedingly real, and capable of giving rise to very dangerous effects. The choice which Professor Forbes has made of his type of line was evidently, as he has told us in the paper, very largely dictated by a desire to avoid these difficulties; and he has told us that one object in designing this line for the transmission of power to this distance from Niagara to Buffalo was to so construct it that these capacity effects might be as far as possible reduced. One of the great risks which has to be avoided in the construction of such lines is the presence of pressure effects, which take place when the lines are suddenly connected to or disconnected from the machines. This ought never to be done in practice. Professor Forbes is quite right in saying that under these circumstances electrical surgings are set up in the cables and in the machines, and that the pressure may momentarily become almost anything you please. It may become scores, if not many more times, greater than the main working pressure—in fact, it is impossible to say under some circumstances what the pressure actually does become; and under these conditions it is no use to talk about failure of insulation, because there are conditions under which no insulation that could possibly be put on the cable would be sufficient to bear the pressures that are brought against it.

Dr. Fleming.

Dr. Fleming.

Fortunately, however, it is in our power to control these effects, and they are not of the nature of difficulties which cannot be overcome; and the methods by which they ought to be controlled have only to be known, I think, in order to be adopted in every case where we are dealing with long lines which have some, and perhaps a great deal of, capacity, according to the type of cable used, and which certainly necessarily have self-induction when they are connected with transformers at the other end. As Professor Forbes has mentioned, these effects have been got over in the case of the long trunk lines at Deptford. There is no objection, perhaps, to my telling you the methods which have been adopted by me as consulting engineer of the London Electric Supply Corporation, and by my colleague, Mr. W. P. D'Alton, the engineer-in-chief, after the fullest investigation and discussion. These methods have been directed towards avoiding any change of pressure in the trunk mains at the moment they are connected or disconnected with omnibus bar of the machines. Everyone who has experimented with condensers of considerable capacity connected with alternating circuits knows the risk of disconnecting condensers suddenly, and the same result takes place when long concentric cables are disconnected or connected. A very violent spark is generally seen at the moment of disconnecting a long concentric cable from the circuit even when there is no load upon the other end. This, of course, is due to the capacity effect of the cable. The method that has been adopted at Deptford of getting rid of these effects is very simple, and it is as follows:—Let C (Fig. 1) stand for the concentric cable. A is the alternator to which the inner member of the cable is to be connected. The ordinary way of going to work—or, rather, the original way of going to work—was to connect A suddenly by a switch with C. Of course that was a disastrous method of doing it, and it was a long time before the actual reason of these cable failures was discovered, and also the way to prevent them. The method now used is as follows:—The cable, C, is never suddenly connected with A, but a transformer, T, is placed in position near the main, and that transformer has on its secondary circuit, S, a variable resistance, R, by which it may be gradually short-circuited. A water or carbon

resistance is generally employed, or any other way in which the secondary circuit of the transformer can be gradually short- Dr. Fleming.

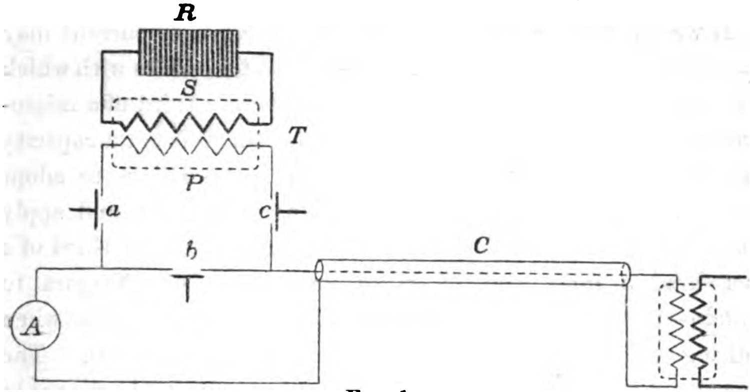


FIG. 1.

circuited. Then there is an arrangement of three switches, as shown in the diagram. We will call these switches *a*, *b*, *c*, in order to distinguish them. The first process is to keep the switch *b* open, and to close *a* and *c*. The transformer is then short-circuited gradually on the secondary switch. Under these circumstances the transformer offers a very considerable impedance to the flow of the current, and that prevents any sudden rush into the main. If there is sufficient impedance no sudden rush takes place into the cable: it is under complete control. Under those conditions there is no impulsive effect in the cable; and the next step is to completely short-circuit the secondary circuit of the transformer, and to thus annul the impedance of the transformer, getting rid of it little by little, as slowly as you please. When it is completely short-circuited the switch *b* is closed. Under these circumstances the capacity current running into the cable is brought up from nothing to its full value as slowly and deliberately as you please. Then the transformer can be disconnected by switches *a* and *c*, and the cable put directly on to the main. The process of disconnecting the cable is exactly the reverse. You must never sever the main switch instantly. First of all, the transformer must be connected on whilst short-circuited on its secondary side. Then the switch *b* is opened, and then the short-circuiting of the secondary is gradually removed. In that way

Dr. Fleming.

the impedance is brought into the circuit gradually, and the current is, as it were, stopped from running into the cable.

If we are dealing with large cables the capacity current may reach a very considerable value. Most concentric cables with which one has to do seem to have a capacity of about a third of a microfarad a mile. It seems impossible to get one that has not a capacity something like that, if it is concentric. If we were to adopt a pressure of 20,000 volts, with a frequency of 100, and apply this to a concentric cable which had a capacity of a third of a microfarad a mile, and 18 miles long—say from Niagara to Buffalo—the capacity current alone would be 72 amperes when nothing was connected at the other end of the tube. The capacity of that capacity current to do mischief is proportional to the square of the capacity current, and that is itself in proportion to the frequency; and therefore the advantage of adopting a low frequency and a low capacity is something enormous when you come to these long lines. In fact, anyone who has had any experience in working large concentric cables would hesitate to adopt any such type of cable as a concentric cable for a transmission over a distance of 18 miles unless it was accompanied by a careful provision of the kind mentioned, for thoroughly controlling the flow in and out of the cable. These capacity effects are then all increased by increasing the frequency; hence the argument for reducing this frequency in any case in which it is possible to do so. The same class of effects are produced in a certain degree when machines are connected and disconnected in parallel suddenly on to an omnibus bar, and I think there is very great safety in using an artificial load in putting machines in and out of parallel. In all the Continental stations with which I am acquainted, or which I have been able to visit, they invariably use this artificial load for putting machines in and out of parallel; and although this may not be a necessity where you are dealing with machines which have considerable self-induction in the armature circuit, and therefore a large drop, and not so much where you have small self-induction, and therefore a small drop, yet, nevertheless,

I think there is a great advantage in using it always. When I was at Rome last Easter, for the third time, I spent a long and interesting day with Professor Mengarini at Tivoli, and he allowed me to experiment with six Ganz machines, putting them in and out of parallel. They have in one part of the station an artificial line which is made of spirals of iron wire. In this they can take up 300 H.P., which is the power of each of these alternators, and this artificial load is made to resemble as closely as possible the real external line. The process of putting machines in consists, first of all, of putting them on to the artificial line, adjusting the incoming machines so that they may be giving exactly the same current and volts as the machines on the circuit. In one experiment the current going out to Rome was 25 amperes at 5,000 volts. I asked the engineer to put on two machines, and he did so, and we divided the current exactly between the two machines without any difficulty. The third machine was put into parallel, and we divided that, and then a fourth and a fifth machine, until we had all five machines running in parallel, absolutely giving 5 amperes each. They were taken out one by one, and the process was reversed, using the artificial line. That artificial line is also used in all the Ganz stations, and I think Professor Forbes has adopted a very wise course in making a provision for it in dealing with his machines. It is a curious thing that we have not adopted it in England. I do not know of any station in England where the artificial line is used for putting machines in and out of parallel. Yet at the same time I do not know of any large alternating stations on the Continent where they do not use them. Dr. Fleming.

One word upon the question of pressure. I was very glad to hear Professor Forbes say that he never intended the primary circuit to be opened. But the question naturally occurs, Will not the primary circuit sometimes open itself? In other words, What is going to be done in the way of main fuses? because that is really a serious question. In dealing with these pressures, what type of main fuse does Professor Forbes intend to adopt? He tells us in the paper that he is going to have a transformer across the secondary circuit of which is an arc light arrangement.

Dr. Fleming.

I trust that this arrangement will be found to work well; but at the present moment one of the most serious wants in connection with large distribution of alternating currents is an arrangement of main fuse which will not interrupt the circuit, but if the main current goes above a certain value will gradually but quickly put impedance into the circuit and then take it out again as required, but which will never on any occasion allow the main circuit to be broken. One of the great difficulties which arise in working a system of this kind is that the main circuit does become broken, and then a rise of pressure takes place that disastrously affects the insulation of the circuit. The insulation is strained or injured, and afterwards, at some time or other, a failure is sure to occur. Then, with regard to the question of pressure, that, of course, is one to which Professor Forbes and his colleagues no doubt gave very serious consideration before they decided to adopt 20,000 volts. I was told by Professor Mengarini and Mr. Blathy at Rome that they gave that question exceedingly careful consideration when they started the Tivoli-Rome installation, and they decided that they could not see their way to go above 5,000 volts; and they have worked these machines by an overhead line quite successfully over 18 miles at a pressure of 5,000 volts. They discussed this question very carefully in the light of all they were able to learn at that time. The difficulty which may be encountered at this high pressure is not merely the insulation of what may be called the static pressure of 20,000 volts, but the tremendous arcs that may be produced if they are once started. Professor Forbes does not tell us how far apart these cables are to be in this conduit. He described a conduit in which bare copper cables, or copper strips, are to be used as conductors, but I do not think he mentioned what distance these are to be apart. That is a very important matter, because it may be impossible under some circumstances to prevent such a rise of pressure that an arc does start. Then what will be the state of affairs if arcs start between any conductors in this conduit if they are only one foot apart having 20,000 volts between the conductors and 5,000 horse-power at the back of it?

One remark upon the question of frequency. It is sometimes

asked, What is the best frequency? It is very much like asking what is the best speed to walk at. It depends on where you are going, and what you are going to do. I suppose the difference between the frequencies which have been adopted in England, on the Continent, and in America, have been determined by the various conditions under which we have to work. The Continental engineers have not gone at the matter haphazard; they have deliberately adopted a frequency of between 40 and 50 after careful investigation; and I suppose we in England have been driven to 80 or 100 $\sim$ by the circumstances under which we have to start alternating-current work; and the same in America. There can be no doubt about the wisdom of the choice of selecting this low frequency for a case such as the Niagara transmission, in which the question of utilisation of power by motors is the important matter, and in which there are enormous advantages to be gained by the reduction of all these capacity effects by the employment of a reduced frequency. Dr. Fleming

I will not go into other points which are naturally of great interest, but I content myself with alluding to these few matters, and we shall no doubt hear with very great interest what Professor Forbes has to say upon these points in the course of the reply which he will make.

Mr. W. M. MORDEY: I am sure we all feel this to be a most important occasion. We are called upon to discuss a paper—a very able paper, as was to be expected from its author—on a subject of world-wide importance and interest. I believe that everything that is said in this discussion will hereafter be weighed time after time, and that each speaker will speak with a strong sense of responsibility. The practical experience of some of us does not run in the same groove as that of others; but no doubt we shall get the united experience and views of people who work on different lines, and I hope we shall be able to arrive at some reliable and definite conclusion as to whether the new proposals that Professor Forbes has put before us are thoroughly sound or not. What makes the paper especially interesting is that Professor Forbes not only explains this important scheme and describes the works, but he Mr. Mordey

r. Mordey goes pretty fully into the reasons and principles that have led him and his colleagues to decide upon the various fundamental points in the scheme. He puts the evidence and the reasons before us, and I have no doubt he wishes us to test and sift that evidence, each one in the light of his own knowledge and experience. It is very interesting and significant, after so much work has been done on alternate-current distribution of all sorts, that Professor Forbes should come here and practically throw the gauntlet down to the whole industry on two or three points of primary importance in connection with distribution. This paper and this scheme cannot be discussed as a special and isolated affair, dealing only with Niagara. Indeed, the author at the outset points out that it deals with the general question of power-transmission; it deals directly or indirectly with alternate-current transmission, whether for power or light, and many of the considerations put forward as primarily affecting power-transmission are in every way applicable to our ordinary lighting work. This is also of interest because it is extremely undesirable that electric distribution should have to be developed in two separate and distinct ways; it is to be hoped, even if it should be necessary to compromise a little here and there, that it will be found not only possible, but on the whole of advantage, to carry out both kinds of work by one and the same kind of plant and system. That it is not only as a transmission scheme for power that we are asked to discuss these proposals is very evident, for, while very little information is given as to what are to be the actual applications of the power, the author definitely states that lighting work to a considerable amount is to be done, not only at Buffalo, but also in most of the other towns which will be supplied from Niagara (p. 500). Therefore it is a light and power scheme. It is necessary to recognise this clearly, for this very sufficient reason: If the author's principles and conclusions are sound, then they affect us all directly, because they must sooner or later be adopted by us in our ordinary practice. With the general scheme—the distribution by alternate currents, either for lighting or power, using very high tension for long distances—I may say I cordially agree—that is, as compared with

the use of direct currents. I suppose no member of this Institution at this day would think of using anything but an alternate-current scheme for such work. There has been a great advance in knowledge on that subject within the last three or four years; but I think that we have now all come to the conclusion that no large power-transmission scheme can be successfully accomplished for long distances except by the use of alternate currents. This is a simple and obvious matter now; but I believe I am right in saying that long before it was so simple and obvious, and when there was a strong bias against it in influential quarters, Professor Forbes strongly advocated it for this Niagara scheme. As one of those who, before any proposal whatever was made for Niagara, strove to bring into prominence the advantages of alternate-current power-transmission before this Institution and elsewhere, I wish to congratulate him on the acceptance of his views. But, whilst I agree with the general scheme so far as it is an alternate-current scheme, there are certain new features that are advanced which I wish, in all friendliness of spirit, to criticise. I must say I do not think the evidence put forward to justify these departures from established practice are quite sufficient, without further elucidation, to satisfy us. Within the limits of the time allowed in these discussions one cannot do very much. I hope you will bear with me if I exceed the time. Mr. Mordey.

The most important feature in the scheme—the radical proposal that differentiates it from our usual practice—is the adoption of the very low periodicity, and it is several of the arguments advanced by the author on this matter that I ask you to examine, in the light of the facts, as far as facts are ascertainable. Low periods are an advantage in some respects, but in others they are not, and it is necessary that we should carefully examine each statement put forward, and see to what extent it is supported by facts, and how a true balance is to be struck between advantages and disadvantages. Whatever rate is chosen, it must be remembered that it is a compromise. There is no periodicity that combines advantages in all parts of an alternate-current system.

Professor Forbes states—without, however, giving anything

Mr. Mordey. beyond the bare statement—that "of course it is a matter "of common knowledge that parallel working is assisted by "lowering the frequency." With all respect to him, I must dispute this statement. I think there is no evidence whatever to show that lowering the frequency is an advantage in connection with parallel working, except in some inferior types of machines. If with most machines, as he asserts, there is no difficulty whatever in working at 100 periods, there can be no advantage (on this score, at least) in working at a lower rate. It may be that in certain types of machines it may be easier to work at low rates; but this should be taken simply as a proof of the choice of type being unwise, since a type of machine that is incapable of working at 100 periods will work worse at 16 or 40 than a machine made on types that will work at 100 periods successfully. In this matter Professor Forbes relies principally on the authority of Messrs. Ganz, who have written to him that their reason for adopting the frequency of 42 periods per second, was that it was the lowest frequency that would prevent serious flickering of arc lamps, and that their desire was to lower it as far as practicable in order to ensure parallel working (p. 494). Messrs. Ganz are responsible people; they have had considerable experience all over the Continent; but, nevertheless, it is very necessary that we should examine and sift a statement of this sort. I am able, on their own authority, to show that it must be rejected. In the paper which I had the honour of reading before this Institution in 1889 on "Alternate-Current Working,"* I quoted a letter that M. Zipernowsky had written to the *Electrician* of May 10th, 1889 (p. 16), stating that their "low "periodicity was chosen on principle,"—that "we by this "means are enabled to couple our dynamos in parallel is not "a secondary result, but just the end we were aiming at;" so that, like Professor Forbes, I had it in writing that that was their reason for working at a periodicity that had many disadvantages. My surprise, therefore, was very great when in the discussion on

* *Journal*, xviii., p. 587.

my paper M. Zipernowsky wrote: "We state that we have *not* Mr. Mordey.
"reduced the periodicity because of the coupling in parallel. "Since we have commenced building alternating-current dynamos "we have kept the number of 85 alternations, with only slight "variations."* I happen to know that this last statement is actually the correct one. For a considerable time before parallel working was thought of Messrs. Ganz had been making alternators at 40 or 42 periods, or about 85 alternation, per second. When parallel working came along and was recognised as a necessity, they adhered to 42 periods, which had long been their standard, and, by some curious lapse of memory, gave out they had adopted it because it gave the best results in parallel working. Those words, taken from M. Zipernowsky's own writing, show that his last public statement contradicts the first one, and he acknowledges (which we all knew quite well) that they had made these machines for low periods for years before parallel working was thought of. I am not aware of any other support than that contradictory one of Messrs. Ganz to Professor Forbes's statement. Nothing can be more definite than this last statement that they had not reduced the periodicity because of the coupling in parallel. If a reduction of periodicity is necessary to enable any alternator to work parallel, I believe it may be taken as an infallible indication that the type is unsatisfactory in those qualities of good regulation, absence of troublesome reactions, and ability to exert powerful synchronising efforts which have come to be regarded as the criteria of good machines, and which will always be found to be associated with the highest efficiency. While on this subject, I wish to point out that, while many of the troubles of parallel running are due to the uneven turning or defective regulation of the prime motors, the very smooth and even driving that is a feature in turbines ought to make the parallel coupling exceptionally satisfactory. I do not consider, therefore, that, at least on this account, there is anything to show that the proposed reduction is in any way called for.

---

* *Journal*, xviii., p. 672.

Mr. Mordey. The question of the use of an artificial load in putting machines on the mains in parallel has also been dealt with by Professor Forbes, and it has been approved by Professor Fleming. I have considered this method many times, but can find no reason for its employment, and a great many against it. It is the old, primitive, obsolete method used with the early Edison "Jumbo" machines, as they were called, here in the Holborn Viaduct. And in the Milan station, where several of these machines are still at work, a bank of lamps, or some other artificial load, is put on the machine in the way that Professor Forbes fully describes in his paper. I say even with the "Jumbo" Edison machine this is an unnecessary proceeding; and with alternators, relying on a very extensive practical experience, I say it is equally unnecessary. Referring to our own experience, the plan that we have adopted at the Brush Company, and that Mr. Raworth and I got out some years ago, has been quite successful. We regulate the load by the steam, and the E.M.F. by the excitation, bringing the machine into circuit with only enough excitation, and only enough steam to drive it at the required E.M.F. with no load. We connect it when nothing can go out of it or come into it, and then give more steam, so that the load is got on gradually. When taking the load off, we gradually turn off steam and the load falls away, the circuit being opened when nothing is passing. If necessary, if the machine has a reaction, the excitation must be slightly reduced; if there is a large reaction, then it must be considerably reduced. You then get the machine on or off the mains without any spark or shock whatever, and with practically no current on opening or closing the armature circuit. In that way there is no sudden or violent shock; it is a simple, straightforward way, and gives no trouble, and it will be seen that this plan avoids all closing or opening of main circuits. I think Professor Forbes was away in America when I recently read a short paper explaining how we carried out this process in practice.* And even with machines which have a very large drop in

---

* *Journal*, xxii., p. 131

the curve I have found our method acts quite satisfactorily; Mr. Mordey. therefore I am able to say that the method of artificial loading is of no advantage, but an expense, and a source of complication, danger, and trouble. The reason why it has been used on the Continent is that there they are mostly Ganz plants, and I suppose this has become a stereotyped method, and has been put in as a matter of form.

To return to the question of periodicity. There is another point affecting the periodicity to which Professor Forbes referred (at page 498) as probably the most important. He said, "I am not sure that it ought not to be said that "the greatest advantage of low frequency is in connection "with the conductors used for transmission, and in the parts "of the apparatus that require high insulation." Then he refers to what, as he says, has been strongly urged by Lord Kelvin—that effect which was found out independently, as Professor Forbes mentioned, by those wonderful experiments of Professor Hughes—the tendency of the current to avoid the inside portions of the conductors at high frequencies. That is, no doubt, in low-tension alternate-current distribution an extremely important matter. But with these high pressures that Professor Forbes intends to use for the long-distance transmission, whatever other result may come from the reduced frequency, I think I can show that this Hughes effect does not supply any strong argument in favour of the reduction, as the effect is by no means serious. In 1889 (if I may again quote myself), I gave in my paper already referred to a table of the virtual resistance of conductors carrying alternate currents, for which I can claim no credit whatever. I merely tabulated the results which Lord Kelvin had given. It showed the surface effect—this Hughes effect—in various conductors, and for rates of 80, 100, and 130 periods per second, based on Lord Kelvin's figures. The table indicates the limits in the current and the sizes of conductors imposed by the surface effect. Let us see how it affects the Niagara scheme. At 20,000 volts the full current of one section of the machine, two million watts, is only 100 amperes—quite enough to trust to a single conductor. Professor Forbes selects 350 amperes to the square inch as a suitable density.

Mr Mordey. This is less than 0·1 per cent. loss per mile run, and I think a larger density would be more economical. But, taking this density, it will be seen that a conductor of 0·35 of a square inch is required. From my table I find that at 100 periods per second, or six times as high as Professor Forbes wishes to work (not that I am recommending 100 periods), this conductor would scarcely have any Hughes effect in it at all. It would only be increased 5 or 6 per cent. over the ordinary resistance; while even at 133 periods, so common in the States, the Hughes effect is scarcely perceptible. That is to say, the total loss Professor Forbes says he has in the Buffalo transmission is 3·5 per cent. at 25 periods. That loss by the Hughes effect would only be increased to 3·7 per cent. even at 100 periods. Is it worth while going down to 16·6 or 25 periods per second in order to get a gain of 0·2 per cent. in the mains between Niagara and Buffalo? I think not, and I therefore do not find that, on this score, at least, there is any very strong support for the reduction of frequency.

The limit of round solid conductors one can use is about three-quarters of an inch diameter. There are many practical reasons why one should not use a solid conductor, or even a stranded conductor, much more than that section. Several conductors, or, as Professor Forbes once proposed for low-tension work, a flat conductor—a wide strip—would be more convenient. So that even for the 2,000-volt transmission this Hughes effect would hardly tell. It would not be anything like as serious as the impedance resulting from putting the mains one on each side of the tunnel, as appears to be the intention. If these were made concentric, or, better still, the mains put close together, there would be a far greater gain than the loss from this other cause. If I had time, I should be able to show that it would be better to use, say, 50 periods in cables so arranged, than 16 periods and the cables arranged as shown in the drawing of the tunnel. If this is the case, it removes another of the arguments advanced in support of the change.

Then, still keeping to this question of periodicity, we come to the transformers. A long paper would be needed to do justice

to this part of the subject, but if you will allow me two or three minutes I will explain briefly how it presents itself to me now. As transformers to many thousands of H.P. have been made to my designs for various periodicities, and as I have for long been engaged in efforts to obtain the very best results for each set of conditions, I am able at least to speak from experience on this subject. Professor Forbes says the output of transformers is nearly doubled if the periodicity is quadrupled, the hysteresis loss being kept constant. The copper loss would not be less —it would actually be slightly greater as you lower the periods. It is impossible by any amount of money, or space, or time, or thought, to make a transformer as good at a low periodicity as at a reasonably high one. The ordinary idea that a transformer can be designed to be equally good for any given set of conditions as far as periodicity is concerned is an entire mistake. It is not possible. If you take a transformer made for 16 or 40 periods, and run it at a higher periodicity, it will give a very much better effect. You can design a transformer to give the best possible effect with a given amount of iron or copper, and for a given periodicity; but if you increase or decrease that transformer, its efficiency goes down. Professor Forbes acknowledges that by using the low periodicity he loses output and increases cost in the transformers, but he argues that the motors will be more efficient—that this low rate places 3 per cent. more power at his disposal in the motors at the other end. This is a matter that should be sifted to the bottom, and fortunately Professor Forbes gives us data that enable us to do so. Whether for power or light, it will be evident that transformers must be regarded as an integral part of the plant, and the efficiency of the power-transmission must be reckoned on the joint efficiency of transformers and motors. Mr. Mordey.

The author admits that, while the hysteresis loss remains constant for different frequencies, "a given transformer will "give 132 units instead of 100 if you double the frequency. "If the frequency were quadrupled, we should get 174 units "instead of 100." For the purpose of the comparison let us take the latter. Assume that, the frequency is quadrupled; the

Mr. Mordey. hysteresis and the C^2 R losses have remained the same. Therefore the efficiency has risen, because we have nearly doubled the output without increasing the losses. But there is a triple transformation—first, up from the alternator to the mains, then down from the mains to 2,000 volts—he would not in practice go direct to 100 volts—and then down to, say, 100 volts for his motors, because the small motors are, I suppose, not to be worked except at low voltages and with transformers. To get a result based on practice, I may say that at reasonably high frequencies the best efficiency that can be obtained, even for large transformers, is about 98 per cent. At any rate, seeing that we cannot work always at the maximum, we shall be quite safe in so stating it.

Then in a transformer for 174 H.P. we should have 2 per cent., or 3·48 H.P. loss. By reducing the frequency to a fourth, the author takes it that the output would be reduced to 100 H.P., the loss remaining the same; therefore the efficiency is $\frac{100}{103{\cdot}48} = 96{\cdot}6$, instead of 98.

The triple transformation would, therefore, at the low frequency, and on the author's own showing, result in an overall efficiency of $96{\cdot}6 \times 96{\cdot}6 \times 96{\cdot}6 = 90{\cdot}14\,\%$; while the higher rate would give $98 \times 98 \times 98 = 94{\cdot}3\,\%$, or a gain of about 4 per cent. In practice the gain would be a good deal greater, I believe, but at least it would be 4 per cent. on the author's own basis. Now the author considers (page 496) that "the most obvious advantage of low frequency is the improved "efficiency of motors," and he supposes that he gets a motor with 3 per cent. more efficiency than if he used a higher periodicity. The author would not get the advantage in the motors that he supposes; but, even if he did, it will be seen that his own figures show that it would be more than swamped by the losses in the transformers.

As to motors, the author is perfectly clear and definite in his reference to this important matter so far as it affects his decision to use a low frequency, as shown by the quotation just mentioned—"The most obvious advantage of low frequency is

"the improved efficiency of motors." But he cites only a solitary experiment (at page 497) to support this sweeping statement. I am sure that Professor Forbes will acknowledge (not that I wish to be understood as advocating 100 periods) that if thoroughly good motors to work at 100 periods can be commercially produced to have an efficiency of 65 per cent. for a ¼-H.P. motor, of 70 per cent. for 1-H.P. motors, and up to 93 or 94 per cent. for large powers such as 100 H.P., there can be very little to be gained by reducing the periodicity. I am able to state that this is the case from actual experiments: the final tests on the small motors have not been made, but the above figures from the preliminary tests are certainly accurate within 1 or 2 per cent. These results are quite as high as for direct-current motors, and they are the results of tests of practical machines. Indeed, the fact that alternators, either as generators or as synchronising motors, usually give over 90 per cent. efficiency even at the ordinary periodicities seems enough to show that high rates are not destructive of efficiency. Mr. Mordey

I submit that Professor Forbes should modify the passage just quoted: the "improved efficiency" is not an "obvious advantage "of low frequency." There is another statement in the same paragraph (page 497) which I wish to quote and to protest against—"With synchronising motors, of course, the fact has "long been thoroughly established that their performance is very "much improved by using low frequencies." Such positive statements from Professor Forbes naturally carry much weight, but they would carry much more if they were supported by facts. The author gives nothing whatever in the nature of proof, nor even of theory or argument, to support this statement; and, in view of the positive knowledge that many of us have that most excellent results, both as to practical qualities and efficiency, are obtained from existing high-period machines, I think we must ask to be allowed to take this as Professor Forbes's opinion only.

Speaking from facts and experience, and not relying on mere opinions, nor making vague and irresponsible statements, I am forced to place both the transformers and the motors in the list of

Mr. Mordey. things that, when so examined, do not supply any argument in favour of the reduction of frequency.

There is one drawback in connection with the adoption of the very low periodicity that surprises me very much, especially as the author has fully recognised it. I refer to the fact that it cuts off the possibility of directly using the current for lighting purposes, on account of the visible pulsation or flickering. Even if the objections which have been raised against a higher periodicity, so far as power-transmission is concerned, were thoroughly established and sound, I should have expected them to be borne, rather than wilfully to face the acknowledged disadvantages of the low rate for lighting work, which I should suppose would be certain to constitute an important part of the company's work, if only in the lighting of those places to which power is to be supplied.

The example given by Professor Forbes (on page 500)—that of Buffalo—is very significant. Instead of driving the dynamos by motors, it would surely be far better simply to put in synchronous commutators to drive the arc lamps direct, and to use ordinary transformers for the private lighting. Even this latter cannot be done on account of the low frequency. In a scheme of this magnitude the considerations connected with the using up of a few old dynamos in the Buffalo station cannot be worth mentioning as a question of cost. If the cost is so important, the stronger the argument against this scheme, for, instead of putting in transformers at 4 dollars per H.P., it will be necessary, on the author's own showing, to put in motors at 20 dollars per H.P. (probably with transformers in addition). In view of the author's reference to the use of this power for lighting, not only at Buffalo, but also elsewhere, it appears that the use of the power for lighting may be considerable. To all this work, now and always, this low period places a barrier so far as direct use is concerned. I wish to quote the passage (page 500) wherein he tells us that this fact—of the current being too low in periodicity for electric lighting—"is apt to impress one as almost fatal to "the employment of very low frequency." I would only say—not almost, but altogether.

Before concluding, I wish most distinctly to state that I am

not arguing in favour of 100 periods, or any other rate. I am merely pointing out that certain of the arguments advanced to justify 16·6 periods (for the 25 periods is a compromise apparently distasteful to the author) will not bear examination, so far as I understand them. But, while on this subject of flickering, I would say that, in my opinion, even 40 periods is a good deal too low for arc lighting by ordinary alternate currents, as anyone may see by twirling a walking-stick rapidly under such an arc. This is quite enough to show the very marked discontinuity of the light. Mr. Mordey.

Although my company has never made alternators for more than 3,000 volts (we have transformed up for higher pressures), I have frequently had to consider the question of high E.M.F., and I must say that I quite agree with the manufacturers and engineers who have (as Professor Forbes tells us on page 504) resisted his efforts to induce them to make 20,000-volt alternators. Of course much depends upon the type of armature, but with the type adopted I do not think it would be advisable, and in any case I feel sure it would be cheaper to transform up. But I do not see why you should transform up from 2,000 to 20,000. 2,000 volts is not as safe as 500, and it is as easy and cheap to transform from 500 to 20,000 as from 2,000 to 20,000. The alternator would be safer to work; it would be no more difficult to wind (since various sections can just as easily be placed in parallel as in series); and the conductors for the large current for the short distance between the alternators and the transformers present no difficulty whatever. But, even if alternators could be made to give 20,000 volts directly, I think it better to transform up, for this reason: the space and ventilation required to get safety and efficiency in very high tension apparatus of this kind are provided in transformers more easily than in the armatures of alternators. The alternator for high tension is more costly to construct than for low, and a given machine is safely capable of a larger output at a low tension. And even more important is the immunity from breakdowns that is secured in generator or motor by transforming. The author is determined to have no breakdowns of insulation; but I think that, however good the

Mr. Mordey. plant, and however low the E.M.F., one must be prepared for occasional breakdowns of insulation—at least, I know of no machinery that is absolutely infallible. A breakdown of an armature means a stoppage at least of that machine; whereas, when transforming up with a number of transformers, then a failure of one transformer is unimportant. The transformers would be arranged parallel, and the offender would simply be cut out of circuit by its fuses, or otherwise, in the ordinary way, and no harm would be done.

A point of very great interest to many people who have been considering multiphase working is the bad regulation of the machines when the load is varied, as Professor Forbes has pointed out at page 492. I once tested what I think was the first multiphase machine—although not made for what we now call multiphase working—an old Gramme machine of the sort used for the Jablochkoff light. It was a very bad regulator. If you switched load off one side, the volts on the other went up enormously. That is one of the difficulties experienced in modern multiphase machinery. I believe it is a question of design. If we used machines of types that as ordinary alternators give a reasonably straight characteristic, they would not have this bad effect. I have made a two-phase machine with half the armature set round, so as to get the necessary displacement, and that machine regulates perfectly. It has only a 4 per cent. change if you take off the whole of one side. The ordinary alternators having curves that come down like the Falls of Niagara are not well suited for either multiphase or single-phase, or any other kind of working whatever where good regulation is required. As to capacity effects there is a great deal to be learnt. We shall learn some day, I hope, to play off capacity and self-induction against one another, instead of accepting them as necessary evils. At present there is rather a disposition to use capacity as a scapegoat for all sorts of things that have not been thrashed out fully. I trust that later on Professor Forbes will be able to add to our knowledge on this subject. At present my experience, I regret to say, is that the capacity for holding a charge of dirt is the most important of all capacity effects in a dynamo, and causes far more breakdowns than anything else.

Mr. GISBERT KAPP: The Institution ought to feel proud that one of its members has been selected to carry on this most important work. I am quite sure we all feel that it is an honour to ourselves, and I commence my remarks with this tribute to the author, in order that he may not think my judgment biassed in any criticism that I may offer. If I am biassed at all, I am, of course, biassed in his favour. The first point which claims attention is the number of phases. When I was in America in the summer, I had the advantage of discussing this matter with Professor Elihu Thomson, who tried to convince me that three phases would require less copper than two. After my return I studied this question, and found that Professor Thomson was right. When comparing different systems it is of course essential that this be done on the same basis for all. The basis of comparison I took was the practical and necessary condition that all the systems compared should have the same safety as regards insulation, or, in other words, that the stress on the insulation should be the same. Now if we take the continuous current as the most economical method of transmitting a given amount of power under a given stress on the insulation, we may take the weight of copper required in the line as the irreducible minimum, for it is obvious that with a continuous current the copper is best utilised. To get the same stress on the insulation with an alternating current, we must select such an effective pressure that the maximum pressure, or crest of the wave, does not exceed the pressure of the equivalent continuous current. The permissible pressure will therefore depend on the shape of the wave, and, taking this to be sinusoidal, we find that the single-phase alternating current will require twice as heavy a line as the continuous current. If the copper in the line weighs 100 tons for a continuous-current plant, and you wish to work another line, but of the same length, and giving the same efficiency of transmission and the same stress on the insulation, with an ordinary alternating current, you will use 200 tons. If you work the two-phase system with four wires, you will again use 200 tons; but if you think you are going to save copper by using only Mr. Kapp.

Mr. Kapp. three wires and working the two-phase system on that combination, you will use, not 200, but 290 tons of copper—the increase of weight being due to the fact that the potential of two of the four terminals is forcibly made the same, which compels you to reduce the effective pressure in each circuit; so that the system which Professor Forbes recommends as the most economical in copper is actually the worst. If you now had three wires, and worked them on the three-phase system, you would be using only 150 tons of copper. These figures give, therefore, the merits of the different systems. I should like to draw Professor Forbes's attention to the question of how the output of transformers varies with the frequency. He does not give the way that he arrives at his conclusions, but states that, if you assume the Steinmetz law that the hysteresis loss is proportional to the induction raised to the power 1·6, the output of the transformer varies as the frequency raised to the power of 0·4. I make it 0·375, and that is rather in favour of Professor Forbes's contention that the low-frequency transformer does really not become so very much heavier or costlier than the high-frequency transformer; but on the question of efficiency I join issue with Professor Forbes. In this respect I quite agree with Mr. Mordey that no amount of expenditure will enable you to make the low-frequency transformer as efficient as the high-frequency transformer. The reason is obvious: in lowering the frequency you reduce the output, but you do not reduce the loss.

As regards the motors, I have again the misfortune of differing from Professor Forbes. He thinks the low-frequency motor will be more efficient, and he gives a calculation at the end of his paper to show that the losses in the high-frequency motor are 42 per cent. greater than in the low-frequency motor. I quite agree with the 42 per cent. increase, but I am afraid Professor Forbes has overlooked the fact that the high-frequency motor under the conditions stated in the paper will require the supply current to have 2½ times the electro-motive force, and it will give 2½ times the power. When this is taken into account, what appears to be an increase in the loss of 42 per cent. becomes actually a gain of 33½ per cent.; so that on the author's own

showing the high-frequency motor is the more efficient of the two. Leaving, however, theory aside, I may say I know of no example except his own on record which will show that there is a gain of 3 per cent. by lowering the frequency within these limits. One reason that he gave for the use of the low frequency, and which appealed to me very strongly at first, was that you can make a synchronising motor out of any commercial dynamo. Suppose, for instance, you want to rig up your power quickly, you can go and buy a dynamo and set it up to work. This seems very nice on paper; but when I came to consider what had to be done to the so-called commercial dynamo, I found that it was not so simple. You have to put two contact rings on to begin with. Nowadays dynamos are cut pretty fine, and makers do not give room for fancy fittings. You will have to send the dynamo to a workshop to be fitted with these rings and additional brushes. Suppose you have done so, and you have put it down ready for work, how are you going to start it? The thing won't start by itself, because it is a single-phase motor. You must put down a little Tesla motor or some other alternate-current motor to start it. If you do that, you may as well use the Tesla motor a little larger and run your factory from that. Single and multiphase alternating-current motors are in the market, and are very good. The impression that they are not to be obtained as a regular article of commerce, and that one or two which might perchance be obtained are rather of an experimental character, and not reliable for practical work, is simply due to our technical electrical Press. I am afraid our electrical papers are serving us badly in this respect. Among the English electrical papers it is the fashion to cry down multiphase and alternating-current work; and although I can sympathise with the editors, who must find it a very inconvenient thing to have to take up new subjects of study so as to be able to write intelligently about them, yet I think we have a right to expect that our engineering papers should keep us informed on all subjects of practical importance to the profession. That the subject of alternating-current motors is of great practical importance is amply proved by the paper we are discussing. I may mention that only four weeks ago I visited Mr. Kapp.

Mr. Kapp. the Œrlikon Works, and was shown a large number of single-phase self-starting motors now made as a regular article of commerce. One of these motors I tested very carefully, using a brake and wattmeter, and I found the performance remarkably good. The machine gave on the brake 862 watts. It ran at a speed of 1,850 revolutions per minute. The frequency varied from 60 to 67; the magnetic slip was 4 per cent., commercial efficiency was 60 per cent., and the power-factor 74 per cent. At starting, the current to produce a torque equivalent to the full power (1¼ H.P.) was 2¼ times the normal working current. It is rather inconvenient to have such a large current at starting, but by a very ingenious arrangement, now used all over the Continent, the supply mains are not called upon to furnish the excess current at starting. This arrangement consists of an auto-transformer with multiple contact switch, so connected up as to transform a small current at full pressure into a large current at reduced pressure. As soon as the motor has attained full speed, the transformer is switched out and the motor receives the supply current direct. The weight of the machine is under 1 cwt. per H.P., and in this respect, as well as in point of cost and efficiency, the motor I tested is quite equal to, if not actually better than, a continuous-current motor of equal power.

The adoption of so low a frequency as 16⅔ cycles per second is a matter of surprise to most of us. There are very strong reasons against the adoption of a low frequency as regards motors—not quite so strong reasons as regards transformers, but still sufficient, but there is a very strong reason on account of the increased cost of the alternators. I have studied the design of Professor Forbes's alternator very carefully, and I may say at once that the machine appears to be of a most excellent type. I am very pleased to be able to say all the good I can of his machine, as I have unfortunately not been able to agree with Professor Forbes's conclusions in other parts of his paper. It is a most interesting design, and will repay the trouble of careful study. I have made a design on the lines indicated by the author, and was surprised to find what large output and high efficiency it is possible to obtain with a moderate amount of material. I did not know the

dimensions of Professor Forbes's machine, but in my design I found that an armature of at least 10 feet diameter would be required for the 5,000-H.P. two-phase machine. I was pleased to learn from Professor Forbes to-night that the armature in his design is actually 10 feet in diameter. I merely mention this to show that dynamo designing has nowadays become an exact science. The leading features of my design are given in the first column of the table. The second column refers to a machine designed for double the frequency. Mr. Kapp.

The effect of frequency on the general design of an alternator is very marked. Low frequency means that we have few armature coils, but each containing many turns. There will be great armature reaction and great self-induction, resulting in a drooping characteristic—that is to say, a considerable difference between the voltage at no load and full load. This defect—which, apart from entailing more supervision if the voltage is to be kept constant, makes parallel working less certain—can be remedied by generally increasing the size of the machine; but this is an expensive remedy. The result is, that for the same type and the same output the low-frequency machine must always be heavier than the high-frequency machine, up to the point at which a further multiplication of poles introduces a new set of difficulties altogether. There is for every type and size of machine a particular frequency at which it is not only cheapest, but at which the characteristic curve has least drop, and at which two or more such machines will work best in parallel. This point with the type and size of machine adopted by Professor Forbes is very much higher than 16⅔ cycles per second, as a glance at the table will show. In it are given some of the data I have found in designing the machine, first for the frequency adopted by Professor Forbes, and then for double this frequency. You will see that the high-frequency machine has not only less drop, but contains considerably less active material—by which expression I mean the armature iron, the field iron, and copper on armature and field. The total of these weights comes to 95,000 lbs. in the low- and 59,000 lbs. in the high-frequency machine. Taking the weight per E.H.P., we get with low frequency 1 E.H.P. for 19 lbs.,

Mr. Kapp. and with high frequency 1 E.H.P. for 11·8 lbs. of active material.

5,000-H.P. Alternator: 20,000 Volts, 250 Revolutions.

	Frequency.	
	16·66	33 33
Number of poles	8	16
Volts on open circuit	22,200	21,500
Total losses per cent.	2·145	1·660
Armature plates ... ... lbs.	32,000	20,500
Magnet plates ... ... „	12,000	10,600
Yoke ring ... ... „	38,500	18,200
Field copper ... ... „	7,200	7,000
Armature copper ... ... „	5,500	2,800
Total ... ... „	95,200	59,100
Lbs. per E.H.P.	19	11·8

Professor Thompson. Professor Silvanus P. Thompson: I wish to join in the chorus of congratulation to Professor Forbes on the one hand, and to the Institution on the other, that this hall has been selected for the arena for the battle ground, and that Professor Forbes has given us so much food for thought and for discussion. Rather than criticise, I feel inclined to felicitate him on the real step forward that he has brought us in our appreciation of the problem of the utilisation of power from such a source as that of the Falls of Niagara. I cannot quite understand the apologetic tone that Professor Forbes seems to adopt all the way through this paper, as if he was a little inclined to give us the reasons why he should not have adopted the continuous current, yet all the while tending in that direction by urging the advantages of low frequency. But surely there is no need at the present day to apologise for not

employing the continuous current for such a purpose as this, when you can do so much more with an alternating current than with a continuous one; when you have so many opportunities of changing it round and transforming it in one way or another, as occasion may require. The paper is indeed a wonderful illustration of the way in which one or other method of transformation may be employed in order to handle the energy placed at your disposal and apply it to various specific ends. Professor Thompson.

The paper bristles with many points of interest, but I would confine my remarks to one or two specific matters. First, concerning the rules for parallel working. The remark is made in the paper that "the rules which govern the construction of "machines that shall work well in parallel are not very clearly "understood. The only fact which has been perfectly established "in practice is that, the lower the frequency, the more easy and "sure the parallel working." I also feel disposed to dispute that latter point: it has not yet been established. It may be true, but I do venture to think that, if it be a fact, it is not the only fact which we possess bearing upon the construction and design of machines for parallel working.

In the first place, we know perfectly well that machines will not run well in parallel if they have weak field magnets, or if they have armatures which, with the output for which the machine is designed, will interfere with the magnetism of the field in any serious way. Further, we know that we do not get, at any rate, easy parallel running between two alternators of distinctly different types. If the alternators are so designed, whether in the placing of their coils, or in the relative breadth of coil and pole-piece, that the curves of their induction are widely different, one giving a form nearly like the sine and the other like a saw-tooth, they will not run well and satisfactorily in parallel. If their curves are very different from one another, you can under no conditions of load avoid having a considerable idle current passing continually to and fro between the machines. I would like to point out that, although we sometimes speak of the self-induction in the armature of an alternator, and of the reaction—the demagnetising reaction—of that armature, as

Professor Thompson.

though they were the same sort of thing (they are in the one sense the same sort of thing), yet they must be carefully distinguished from one another. No doubt whatever, an armature which has no iron in it, or in close proximity to it, will have both less self-induction and less armature reaction than one which has in it much iron, and that iron in close proximity to the coils. But you may have an armature which has large self-induction without necessarily very much armature reaction. It depends on how the iron which is near the armature coil is disposed with respect to the approach of the polar parts. The thing that is disastrous in the design of an alternator for parallel running is not the putting of self-induction into the armature, but the putting of iron into the armature in such a way that the armature reacts violently on the field magnet, and distorts the field so that it becomes unstable. I would ask Professor Forbes whether he has sufficiently considered this point.

There is an expression used in one part of the paper which partially recognises this fact. Professor Forbes says that it is recognised generally that a machine of "stiff" magnetic field works better than one where the iron is far below the magnetic saturation. In a paper read by Mr. Kapp six years ago attention was drawn to this armature reaction, and to the way in which one might describe it graphically by drawing load-curves. Mr. Kapp gave us the curve of excitation of the machine when no current was going through the armature, and when a current of given strength was going through the armature. Similar load-curves can of course be drawn out for other machines. It is quite certain that any alternator which when a large load is put upon the armature requires very much more excitation in the field magnet than it does on open circuit, will give trouble. All this, of course, is implicitly contained in those equations on the action of alternating-current machines which Dr. Hopkinson published about 1884. But what is the physical action that makes it easy or difficult for two machines to run in parallel when placed on the same circuit? This physical effect is easy to follow if one only can describe it by means of diagrams. If two identical machines

are in parallel, but one of them leads in phase, the current that passes between them, and is superposed, as it were, upon their respective share of the current that they are giving to the circuit, will depend at every moment on the difference of their phase. The leading machine will, in consequence, receive the greater share of the work, for in it the current will be more nearly in phase with the electro-motive force (which depends on the magnetic field), and therefore there will come upon that armature a greater drag in the magnetic field, so tending to retard it. Professor Thompson.

Now suppose the current in these armature conductors is capable, by reason of the disposition of iron, of producing considerable reaction. The current in the leading machine tends to weaken the magnetism of its field, whilst the current in the machine that is lagging in its rotation tends less to weaken, or may even strengthen, its field. The one that wants a strong field to keep it from going ahead weakens its field; that one which is behind, that wants a weak field to enable it to catch up, is strengthening its field,—that is to say, the two reactions precisely tend to defeat the tendencies of the two machines to come back again into synchronism; that is to say, a machine with a big armature reaction will of itself defeat its own tendency to synchronise into parallelism.

It was surely not quite fair to those who have worked on both sides of the Atlantic on this question of synchronising machines, for Professor Forbes to dwell so exclusively on the experience gained by Messrs. Ganz with one type of machine, of one periodicity, and under one set of conditions. We all know the question of working alternators in parallel is very largely a question of engine, and of engine governing. Even in the best types of machines—those which have the stiffest field and the least armature reaction—you may have difficulties if the power is wrongly governed. I wish we could have heard Professor Fleming on that point, because we know he has had experience, and is as well acquainted as any of us with the installations of Rome and Tivoli, and other places where the Ganz apparatus is used. I wish it to be distinctly understood that, in my pointing out that great armature reaction is deleterious to the easy

Professor Thompson

synchronising of machines in parallel, I do not mean that self-induction in the armature must necessarily be absent. If we can separate the two effects,—if the self-induction can be put into the armature in any other way without making the armature react on the field, without having the magnetic operation (in which self-induction consists) completing itself through the field magnets and so distorting the proper magnetism of the field,—then it would not have the disadvantage that it usually has. In fact, under certain circumstances, apart from any difficulties there may be in regulating the pressure at the station, self-induction in the armature may be an advantage rather than otherwise.

I wish to return to some remarks about the difficulties of working stations arising from the sudden rush of current when a machine is thrown in or out. Though in all stations switches are provided for breaking the enormous current, yet one knows in practice one never does break, except by accident, a very large current. We all know that station engineers fall into a way of operating large currents by slowing down machines, or by reducing excitation, or by doing something or other which brings the current down to a small amount before the switch is opened; and conversely before it is closed, so as to prevent a large rush of current when the switch is operated. It is all very well to have switches suitable for breaking a large current in a case of emergency, but the emergency ought never to occur.

With respect to alternating-current motors, it ought to be pointed out that that curve of efficiency in proportion to output is not quite to be accepted without reservation. It may be perfectly true for motors of a certain type. The question whether a motor is more efficient with high frequency or low frequency, surely depends on the design of the motor, and on the cause of the losses that lower its efficiency, whether resistance, or hysteresis or eddy-currents. The argument used in the paper was quite irrespective of the design of the motor—that it would be more efficient at a low periodicity; and I do not think that is proved, or even true.

Lastly, I want to congratulate Professor Forbes that he has

dared to introduce a very useful Americanism. He has been called to establish at Niagara a central station, but he has dared to call it, as the Americans call their stations, by the much more descriptive name of "power-house." Central stations are very often eccentric, and the word power-house is a much more apposite term. Professor Thompson.

Mr. SWINBURNE: We must all feel very grateful to Professor Forbes for his able and interesting paper. Before discussing the electrical part, I would like to ask Professor Forbes a few questions. What is the power going to cost the consumer? who are going to use it? and what are they going to do with it? The cost of power to the consumer is doubtless settled, as we hear that villages or cities are being erected to utilise the available supply, and the 'cute American does not build cities to use up power without knowing the price of it first. Professor Forbes has omitted to give us the price, which is the most important question of all. The popular notion that power from waterfalls costs nothing is very wide of the mark. Apart from questions of rent, the interest and depreciation is heavy, and the cost of distribution and collection may be very heavy. People are apt to consider that coal is the only expensive item in the generation of power. Then there is the question of rent, which is overlooked in most cases. As soon as the value of water power is fully understood, owners of water power will, in the ordinary course of business, charge rent that will swallow up the saving by coal. The consumers of power, therefore, do not in the long run benefit by the use of water power. Mr. Swinburne.

The question as to who is going to use the power, and what it is to be used for, is also of importance. In most trades the cost of coal for generating power is a very small percentage of the cost of production. In flour-milling, for example, it is a comparatively small percentage. On the other hand, in pulping wood, or refining copper electrically, it is a considerable item.

This question has another bearing. The whole question of what method of distribution should be employed depends on whether the consumers want large, medium, or small powers. For very small powers, little motors with laminated fields will

Mr. Swinburne. do; but it is not the least likely that much revenue will be derived from very small powers. Medium powers, such as from one to ten horse-power, are used for printing machinery, small workshops, and such things; but it is scarcely likely that much revenue will be derived from such sources in the neighbourhood of Niagara. (By the way, as I am a civilised engineer, and not a North American Indian, I prefer to pronounce Niagara in the usual way.) It is for medium powers, such as one to ten horse, that the double-current system has met with what success it has had so far. Its real advantage consists solely in ease of starting; and this advantage is important in medium-sized motors only.

With regard to the enormous infrequency—namely, 0·04 second per period—advocated by Professor Forbes, he does not make out a very strong case. The objection to low frequencies is that it makes transformers large, expensive, and inefficient, and that it cannot be used for lighting. With regard to the last point, Helmholtz gives 25 periods per second as the lowest frequency which does not give rise to trouble from flickering; while electrical engineers observe flickering with an alternating current of some 48 currents per second, which corresponds to a flicker frequency of 96, or nearly four times Helmholtz's figure. Professor Forbes gives no very sound arguments in favour of low frequencies. He merely refers to well-known facts about parallel running and motors. Well-known facts should not be introduced in scientific discussions. When anyone has recourse to a well-known fact, you always know he is wrong.

If the power is to be used for such purposes as wood-pulping, the motors must be large. There is then no difficulty in starting the motors, and there is no advantage in low frequencies, and none in multiple currents, or, as Professor Forbes calls them, multiphase currents. I would like to protest against the American misuse of the word "phase." The term comes from Astronomy, and to talk of generating phases, as in the paper, or of phasing alternators, is a misuse of the term. The argument that low frequencies permit the use of makeshift motors is very weak. It is to be hoped a scheme like this is not to depend for

its success on ordinary direct-current machines with slip rings fitted on for the occasion. As to the efficiency of motors being greater with low frequencies, Professor Forbes indulges in the very common fallacy of an illegitimate comparison. You cannot take two motors which are the same in every respect but the frequency, and the result of the comparison depends altogether on the alterations which are involved in an alteration of frequency. A Corliss engine will not run well at 500 revolutions, but that does not prove that low speeds are best for steam engines. Mr Swinburne.

Professor Forbes's idea that we unfortunate dynamo designers put on 20,000-volt insulation to stand 2,000 volts is very curious. The trouble with the capacity of the mains on switching off affects the mains alone, and not the dynamo. No dynamo designer designs for ten times the pressure wanted. My company tests with double pressure, and is pleased and proud if the insulation stands. There seems to be a general misunderstanding about the danger from capacity. I was the first to explain the rise in the Deptford mains as being due to a leading current acting partly on the transformer, but chiefly on the fields of the dynamo. Messrs. Glazebrook, Kapp, and Fleming assumed that, because I explained the action with reference to a real transformer and a real dynamo, I must be quite unable to understand the ordinary methods of dealing with the phantom known as electric resonance, which involves K and L and a certain number of elementary differential equations and clock-face diagrams. They therefore explained the action of capacity resistance and L in series at great length, and similar explanations have been breaking out like influenza in various parts of the globe ever since. The Deptford main effect was fully explained quite recently in the States after this fashion. Professor Forbes does not seem to realise that capacity, if treated kindly, is his best friend. If you reduce the frequency of the dynamo, and increase the armature turns in proportion, the rise of pressure due to the capacity is the same. Capacity in the cables also supplies the lagging current needed by nearly all the alternating motors now in use.

The danger to insulation on switching off is of course reduced with the frequency, but the leads should never be switched off;

Mr. Swinburne. they should always be on one dynamo at least, so this does not come in.

Before sitting down, I should like to mention that double-current transmission is not so new to electrical engineers as generally imagined. It was known in England early in 1887, and as I had nothing to do with it I have the more pleasure in referring to it. An inquiry for a large power transmission for Australia came to Messrs. Crompton & Co. Mr. R. B. Rogers, then my assistant there, proposed to use ordinary four-pole dynamos with a double alternating current and four wires. He was quite alive to the extra output, and the reduction of Foucault current and hysteresis loss in the fields. I do not think, however, he contemplated the motors being self-starting. This was a low frequency (33 periods, or less) system; and at that time we thought a low frequency best. Practical experience has, however, since shown us the error of our ways.

Mr. Walker. Mr. S. F. WALKER: There are just one or two points on which I should like to say a few words. I can confirm all that Professor Forbes has said about the winking of lamps. When you work with heavy cranks and single engines, the lamps blink as the cranks fall over. Also, when you have a badly jointed belt, the lamps blink when the joint passes over the pulley. The remedy is to use thick filaments. If you can make your filament as thick as you like, you may make your frequency as low as you please. Large incandescent lamps do not blink. I do not know that I need go into the cause. When you get a thick filament, a small increase of current, giving rise to a small increase of temperature, is not perceptible—it is lost in the filament; whereas, when the filament is thin, the increase is immediately apparent. A decrease of current is immediately apparent with a thin filament, from the greater proportional extent of radiating surface. Then, with regard to the culvert, I should like to ask Professor Forbes what the cost of this half-mile of culvert is going to be. It has appeared to me for some time that for distribution of current in towns you will have to come to that. If you place your cable underground, you cannot be certain what is going to

happen to it. Sooner or later there will be a fault developed. Mr. Walker. Experience in connection with telegraphic wires has shown that most conclusively; and I feel confident that, sooner or later, subways will have to be made for town work where the conductors will have to be carried, and where a man can walk upright and examine them. Of course the subways may be used for a great many other things. I am very pleased to find that Professor Forbes has taken the bull by the horns in this case. The only question is the cost, and it will be very important to know what that is. As to the speed of the dynamo, the revolving parts of the dynamo are very heavy. I give the expression for what it is worth: the speed seems to be high. I followed the design very carefully. You can provide for the speed, for the centrifugal force, for the twisting strain on your spindle—which is the most important of all—by providing sufficient strength in your spindle; but when you come to provide your large steel spindle to stand this enormous twisting strain, you can never be quite certain what material you get in the interior of the steel casting. I think I am correct in saying that, in this country at any rate, all machines in which heavy masses of metal revolve, do so at a very much lower speed—something like one-tenth.

The PRESIDENT: I need not say we shall be only too glad to receive observations in writing from gentlemen who may not be able to be present at the next meeting, when this discussion will be continued.

I have to announce that the scrutineers report the following candidates to have been duly elected:—

Members:

Thomas Octavius Callender.	Thomas Preece.
Ormond Higman.	

Associates:

Arthur Ellis.	R. C. Holness.
John Gordon.	Frederick W. E. Jones.

Students:

Ernest Arthur Bayles.	Herbert William Miller.
Hugh Capper Crawhall.	Drogo Montagu.
William Alan Fraser.	Evelyn Henry Turrell.

The meeting then adjourned.

The Twenty-second Annual General Meeting of the Institution was held at the Institution of Civil Engineers on Thursday evening, December 14th, 1893—Mr. W. H. PREECE, President, in the Chair.

The SECRETARY read the minutes of the Ordinary General Meeting of November 23rd, which were approved.

The names of new candidates for election into the Institution were read and ordered to be suspended.

The PRESIDENT: Gentlemen,—On the last occasion when we met together it was my regret to have to refer to the great loss that the Institution had sustained in the death of one of its active members. It is now my painful duty to refer to a loss which not we alone, but which England, and science throughout the world, have sustained by the death of Dr. John Tyndall. In the early days of this Institution he was a frequent attendant here. I do not remember that he ever took part in our discussions; but I do not suppose there is a single member of this Institution who has not, at some time or other of his life, been indebted to his works for some of the scientific knowledge that he may now possess. And those of us who were frequent visitors at the Royal Institution can recall with the liveliest gratification the memory of the wonderful way in which he used to perform his experiments, and the unrivalled manner in which he was able to make his subject clear to even the most childish brain present. Having lost such a distinguished man, we could not possibly part without making some reference to that loss; and, with the approval of the meeting to-night, I would venture to suggest that, in the name of the whole of the Institution, the Secretary be directed to convey to Mrs. Tyndall the expression of our profound sense of the loss sustained by the Institution and the whole scientific world by the decease of Professor Tyndall, and of our deep sympathy with her in the bereavement she has sustained.

The motion, having been seconded by Professor HUGHES, Past-President, was carried unanimously.

The SECRETARY reported that since the previous meeting donations to the Library had been received from the Director-General of Telegraphs, India; the International Maritime Congress, 1893; Mr. J. McDonnell, Under Secretary and Superintendent of Telegraphs, Queensland; Messrs. Macmillan & Co.; Messrs. Whittaker & Co.; Mr. S. H. C. Hutchinson, Member; and Mr. George Ireland, Associate; to all of whom the thanks of the meeting were unanimously accorded.

Messrs. G. Driver, C. T. Fleetwood, J. Hookey, and J. S. Raworth were appointed by the meeting to act as scrutineers for the ballot for President, Council, and Officers for the year 1894, and for the election of new members.

The SECRETARY then read the Annual Report of the Council, as follows:—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING, 14TH DECEMBER, 1893.

The number of members elected into the Institution during the current year is slightly greater than the number elected last year, and consists of 6 Foreign Members, 18 Members, 101 Associates, and 98 Students, making a total of 223; and 48 candidates have already been approved for ballot at the first meeting next month.

15 Associates have been transferred to the class of Members, and 58 Students to the class of Associates; 1 Student has been transferred to the class of Foreign Members.

DEATHS AND RESIGNATIONS.

The losses by death during the year have, unhappily, been rather higher than the average, and comprise the following 3 eminent *Foreign Members:* Professor van Rysselberghe, Professor Moses Farmer, and Mr. G. B. Prescott, jun.;—12 *Members*, viz.: Sir James Anderson, so intimately and prominently associated with submarine telegraphy; Mr. E. C. Cracknell, chief of the Telegraph Department, and our Local Honorary Secretary, in

New South Wales, one of the most advanced telegraph engineers in that colony; Mr. E. A. Cowper, distinguished as a civil and mechanical engineer, and the inventor of a writing telegraph; Mr. Walter Glover, so well known as the head of the firm bearing his name; Mr. J. E. H. Gordon, who was associated with many electric lighting undertakings; Colonel George Grover, R.E.; Mr. E. T. Mercer; Mr. J. Oppenheimer; Mr. S. E. Phillips, jun., of the well-known firm, Johnson & Phillips; Mr. Anthony Reckenzaun, whose death was alluded to by the President at the last meeting; Professor Tyndall, whose brilliant researches included many important contributions to electrical science; Mr. Henry Weaver, formerly manager of the Electric and International Telegraph Company, and lately managing director of the Anglo-American Telegraph Company; and Mr. J. R. Williamson, of Sydney, New South Wales;—11 *Associates*, viz.: Mr. C. E. Walduck; Captain Rowan; Messrs. A. R. Molison, Ernest Hand, J. W. Howard, S. H. Byrne, E. J. Burt, William Payton, T. H. Brayshaw, Frank A. Bailey, and J. A. Berly;—1 *Student:* Mr. W. Walker.

Since the last Annual General Meeting 5 Foreign Members, 8 Members, 12 Associates, and 11 Students have resigned.

Papers.

Besides the interesting and comprehensive Address of the President, the following papers have been read during the year, some of them having led to very important discussions, viz.:—

LIST OF PAPERS READ BEFORE THE INSTITUTION DURING THE YEAR 1893.

Date.	Title.	Author.
Feb. 23.	On Testing and Working Alternators ...	W. M. Mordey, Member.
Mar. 23.	A New Form of Portable Photometer ...	Sir David Salomons, Bart., Vice President.
,, 23.	Earth Currents in India	E. O. Walker, C.I.E., Member.
,, 23.	Notes on the Influence of Electricity in Tanning Operations	C. K. Falkenstein, Associate.

Date.	Title.	Author.
April 13.	The Distribution of Power by Alternate-Current Motors	Albion T. Snell, Member.
May 11.	On the Prevention and Control of Sparking; Continuous-Current Dynamos without Winding on the Field Magnets; and Constant-Pressure Dynamos without Series Winding	W. B. Sayers, Associate.
Nov. 9.	The Electrical Transmission of Power from the Niagara Falls	Professor George Forbes, F.R.SS. (L. & E.), Member.

These are fewer in number than the average for the past few years, partly in consequence of two evenings early in the year having been occupied by the discussion on Dr. Fleming's important paper, "Experimental Researches on Alternate-Current Trans-"formers," read at the close of the previous year, but chiefly owing to the circumstance that the Institution of Civil Engineers, by whose liberality we are permitted to hold our meetings here, were unable to allow us the use of their lecture hall more frequently than twice a month.

Annual Premiums.

In respect of papers read during the twelve months ending 30th June last—other than those by Members of Council—the following annual premium has been awarded by the Council, viz.:—

The Institution Premium, value £10, to Mr. W. B. Sayers, Associate, for his paper, "On the Prevention and Control of Spark-"ing; Continuous-Current Dynamos without Winding on the "Field Magnets; and Constant-Pressure Dynamos without Series "Winding."

They regret that they do not feel justified in awarding this year either of the other two premiums offered.

Salomons Scholarship.

As announced at the meeting of the 25th May, this Scholarship Fund having been increased by the liberality of the founder —Sir David Salomons—from £1,000 to £1,500, the Council were

enabled this year to grant two scholarships instead of one, and they were awarded to Mr. F. R. Lydall (a student of King's College) and to Mr. J. T. Morris (a student of University College).

STUDENTS' CLASS.

The Council have been glad to observe that the meetings of the Students' class have been more numerously attended than during the previous year, and that the character of the papers read has also improved. With a view of further encouraging these meetings, the Council have decided to offer annually a premium, value £3 3s., for the best paper read by a Student during the session; reserving to themselves, as in the case of other annual premiums, the right of making no award in the event of none of the papers being, in their opinion, worthy thereof.

In respect of the session 1892-93, they have awarded this premium to Mr. W. R. Cooper, M.A., for his paper on "Primary "Batteries in Theory and Practice."

METHOD OF BALLOTING FOR NEW MEMBERS.

On the occasion of the last Annual General Meeting some objections were raised by certain of the members to the method at present adopted for balloting for new members, and the Council willingly consented to give the matter their careful consideration; this they have done, and they have come to the conclusion that any alternative plan that has either been suggested to or has occurred to them, would be open to graver objections than any that have been raised against the present method, which is the same that has been adopted by the Institution of Civil Engineers and the Institution of Mechanical Engineers, and in which they cannot recommend that any change should be made.

BENEVOLENT FUND.

Although the circular inviting subscriptions to this fund has not been so largely responded to as the Council hoped and desired, 126 members (of all classes) have contributed or have undertaken to contribute to it, of whom 11 have agreed to become annual sub-

scribers. The actual amount received up to this date is £646 13s., and a further amount of £78 4s. is promised.

A special committee are now engaged in drafting rules for the government of the fund, and these, when approved by the Council, will be submitted to the subscribers for adoption.

Annual Conversazione.

The President's Conversazione was held at the Royal Institute of Painters in Water Colours on June 23rd, the attendance being considerably larger than on any previous occasion.

Annual Dinner.

The fifth Annual Dinner of the Institution, which took place on the 22nd November, was also numerously attended.

Building Fund.

Although the Council hope that the Institution, by the continued liberality of the Institution of Civil Engineers, may enjoy for many years to come the privilege of meeting in this hall, they feel that it is but prudent to provide for the time when it may become necessary that we should possess premises, with lecture hall, of our own; and they propose, therefore, to transfer a portion of the invested funds of the Institution to a Building Fund, and to add thereto each year a certain amount out of that year's surplus revenue. The matter is still under consideration.

Financial Position.

The financial position of the Institution continues to be satisfactory, and it is believed that when the annual accounts are made up the income will prove to have exceeded the expenditure by a substantial amount. During the year £204 10s. (two hundred and four pounds ten shillings) has been invested on account of Life Compositions, and £1,200 (twelve hundred pounds) on account of the General Investment Fund.

THE LIBRARY.

REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year number 62; of these, 7 were purchased, and the remainder are presentations from authors or publishers.

The specifications of all electrical patents continue to be supplied to the Institution, by the kindness of H.M. Commissioners of Patents.

The number of patents applied for this year, up to the 30th November, was 22,684, of which 1,185, or 5·22 per cent., were electrical.*

The corresponding numbers last year were 21,656 and 1,284, or 5·92 per cent.

The number of periodicals and printed proceedings of other Societies received regularly is somewhat larger than last year, as may be seen by the list appended hereto.

The number of visitors to the Library during the year has been 645, of whom 106 were non-members.†

The corresponding numbers last year were 609 and 95 respectively.

F. H. WEBB,
Secretary.

APPENDIX TO SECRETARY'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE INSTITUTION.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
King's College Calendar.
Liverpool Engineering Society, Proceedings.

---

* Up to December 31st the number applied for was 25,102, of which 1,318, or 5·25 per cent., were electrical.—SEC.

† Up to December 31st the numbers were 677 and 111 respectively.—SEC.

Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal Society, Proceedings of.
*Royal Society, Philosophical Transactions of.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
Canadian Society of Civil Engineers, Transactions.
Franklin Institute, Journal of.
John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.
Technology Quarterly.

FRENCH.

Bulletin de l'Association des Ingénieurs Électriciens sortis de l'Institut Électro-Technique Montefiore.
L'Académie des Sciences, Comptes Rendus Hebdomadaires des Séances de.
Société Belge d'Électriciens, Bulletin de la.
Société Française de Physique, Séances de la.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin de la.
Société Scientifique Industrielle de Marseille, Bulletin de la.

LIST OF PERIODICALS RECEIVED BY THE INSTITUTION.

ENGLISH.

Electrical Engineer.
Electrical Plant.
Electrician.
Electricity.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Industries.
Invention.

* Presented by Professor D. E. Hughes, F.R.S. (Past-President).

Lightning.
Nature.
Philosophical Magazine.
Telegraphic Journal and Electrical Review.

AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.
Electricity.
Journal of the Telegraph.
Science.
Scientific American.

FRENCH.

Annales Télégraphiques.
L'Électricité.
L'Électricien.
L'Industrie Électrique.
Journal de Physique.
Journal Télégraphique.
La Lumière Électrique.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

The PRESIDENT: It is now my pleasure to move—"That the "Report of the Council as just now read be received and adopted, "and that it be printed in the Journal of the Institution."

Mr. ANSELL seconded the motion.

The PRESIDENT: Does any member desire to make any observations on this Report?

Mr. W. T. ANSELL said he rose, in reponse to the President's invitation, to ventilate a question which was a somewhat burning one with that considerable section of the members of the Institution who were immediately connected with telegraphy, and who felt that neither they nor those connected with telephony were adequately represented on the Council. He, and those on whose behalf he was speaking, were quite ready to admit that in point of scientific knowledge they could not claim to be on

equality with the large number of able men who have joined the Institution of late years; but they contended that the fact should not be forgotten that it was the telegraph engineers and telegraphists who founded and maintained the original Society which had grown up into the present important Institution, and on that fact they laid claim to be more adequately represented on the governing body, the Council. Mr. Ansell read the terms of a memorial, which was now in course of signature, representing the views he had endeavoured to express, urging the due representation of all classes of the profession, and which would shortly be presented to the Council. He claimed to speak on behalf of the telegraphic members, because he was the sole living representative in active harness of those who were associated with telegraphic enterprise in 1845, prior to the incorporation of the Electric Telegraph Company. Five only of these men were now living, and one of them he was glad to see present that evening, his old friend Mr. J. W. Wilkins, who was engaged in the construction and equipment of the Northampton and Peterborough Railway in 1845.

The PRESIDENT: I think, Mr. Ansell, that you will find, not only the Council, but the main body of the members of the Institution, to be quite with you in this matter; and if the memorialists will appoint a deputation to confer with the incoming Council, I have little doubt that an arrangement can be arrived at which will be mutually satisfactory.

Mr. ANSELL expressed his acquiescence in this suggestion.

Mr. MADGEN suggested that the Annual Report might deal with some of the many important questions affecting the well-being of the industry, and mentioned as one of them the necessity of dealing with the present very unsatisfactory conditions under which much of the wiring was being carried out.

The PRESIDENT: It is within the province of any member to submit any such questions to the Council, who are always ready to deal with them if they come within the scope of their proper functions.

Mr. GROVE and Mr. S. EVERSHED suggested that advance proofs of the papers to be read at the meetings of the Institution

should be placed in the hands of the members oftener and earlier than is now the case, and the latter gentleman cited the practice of the Institution of Civil Engineers.

The PRESIDENT explained the reasons which rendered it often impossible to circulate such advance proofs. The Institution of Civil Engineers was very differently situated in many respects, but chiefly in the fact of their having what he might call a plethora of papers, and being thus able to have them in type long before they were set down for reading.

The PRESIDENT: If no other member has any remarks to make, I will now put the question that the Report of the Council be received and adopted, and that it be printed in the Journal of the Institution.

The motion was carried unanimously.

Dr. FLEMING: Mr. President and gentlemen,—On these occasions it is only natural and proper that we should record our thanks to those individuals and to that body to whom we are indebted for facilitating these meetings, and for thus enabling us to conduct the most important part of the business of this Institution with comfort and convenience. Although many motions which are proposed on these occasions savour somewhat of formality, the one which has been entrusted to me is always sure of a hearty reception, because it represents real gratitude for a real favour. Although this Institution represents such large and increasing interests in its industrial and scientific sides, and has acquired a world-wide name, yet it has not yet acquired a local habitation in the sense of having a building large enough to accommodate us in these meetings, and we are indebted to that older society which possesses this building for the benefit of being able to meet in this hall. We therefore very gratefully desire to record our thanks to the Institution of Civil Engineers for the privilege which they have so freely given us now for so many years, of assembling in this theatre, and of holding our meetings with such comfort and convenience. I beg therefore to propose—

"That the members of this Institution desire to offer to the "President, Council, and Members of the Institution of Civil

"Engineers their cordial thanks for so kindly and liberally "granting the use of their lecture hall for the meetings of "this Institution."

Mr. HOWARD TASKER having seconded the motion, it was carried unanimously.

Mr. SIEMENS: It is my pleasant duty to propose another vote of thanks to those to whom also we owe a debt of real gratitude, viz., to the Local Honorary Secretaries and Treasurers in foreign countries. You know the Institution of Electrical Engineers was founded with the intention of being of an international character, and up to this time it has very well kept up that character, which fact is to a large extent due to the exertions of those gentlemen who represent us in the various countries and colonies, and have kept up relations with this Institution. I am all the more glad to be able to move this vote of thanks as we have with us this evening one of the most zealous and energetic of our Local Honorary Secretaries, viz., Mr. John Aylmer, Local Honorary Secretary in France, who has occupied that position since 1875. I am quite sure you will all join with me in resolving—"That the thanks of "the Institution are due to the Local Honorary Secretaries and "Treasurers for their kind services during the past year."

The motion, having been seconded by Mr. EVERSHED, was carried unanimously.

Sir DAVID SALOMONS: Mr. President and gentlemen,—I have to move—"That the thanks of the Institution are due to "Mr. F. C. Danvers and Mr. Augustus Stroh for their kind "services as Honorary Auditors during the past year." I am quite sure you will give this vote of thanks with all the more pleasure when you know that there is a very large balance this year—larger than for many years past. I hope we have not given the Auditors much trouble. As the time is short, I will not make any further remarks, but will merely ask you to give them a hearty vote of thanks.

Professor HUGHES seconded the motion, which was carried unanimously.

Mr. MORDEY: The duty I have to perform to-night is a very singular one in connection with electrical engineering. I think

this the only occasion on which electrical engineers have to thank the legal profession for doing anything for nothing. I have to propose a very hearty vote of thanks to Messrs. Wilson, Bristows, & Carpmael for their kind and valuable services as Honorary Solicitors during the year.

Mr. Wright seconded the motion, which was carried unanimously.

The President: I have the pleasure of announcing that the scrutineers report the result of the ballot for Council and Officers for the year 1894 to be as follows:—

President:

Alexander Siemens, M. Inst. C.E.

Vice-Presidents:

R. E. Crompton, M. Inst. C.E.
Sir David Salomons, Bart., M.A.
Sir Henry Mance, C.I.E., M. Inst. C.E.
Professor George Forbes, F.R.SS. (L. & E.)

Ordinary Members of Council:

Major A. H. Bagnold, R.E.
Frank Bailey.
W. B. Esson.
Professor J. A. Fleming, M.A., D.Sc., F.R.S.
Walter T. Goolden, M.A.
Robert Kaye Gray.
Gisbert Kapp, M. Inst. C.E.
Professor A. B. W. Kennedy, F.R.S., M. Inst. C.E.
W. M. Mordey.
Professor John Perry, D.Sc., F.R.S.
Augustus Stroh.
James Swinburne.

Associate Members of Council:

G. K. B. Elphinstone.
The Earl Russell.
Dr. W. E. Sumpner.

Honorary Treasurer:

Sir David Salomons, Bart., M.A., Vice-President.

Honorary Auditors:

Frederick C. Danvers, India Office, S.W.
Augustus Stroh.

Honorary Solicitors:

Messrs. Wilson, Bristows, & Carpmael, 1, Copthall Buildings, E.C.

The PRESIDENT: We have now to resume the adjourned discussion on Professor Forbes's paper. Professor Forbes is departing very soon for the United States, and it will be necessary for us to complete the discussion to-night.

Mr. FERRANTI: I was not fortunate enough to hear the original reading of the paper, but, through the courtesy of the author, I have been able in spare time to read it through and give it a little of that attention which it deserves. With regard to the dynamos, of course it is well known that contractors and designers from all the world over have submitted plans to the Niagara Company. These plans were not found (and this is not a surprising thing) to be entirely satisfactory, but they nevertheless furnished, or are furnishing, a very good basis upon which to design a dynamo. It seems that it would be very hard to design anything that was not in every way the best from such a collection of knowledge as the gentlemen of the Cataract Construction Company have had put before them. There are several important features in which the dynamo proposed is novel. One is the method of insulating the coils. As I have no record to refer to of things that I have said with regard to dynamo construction, I have only the option of referring to the records of the Patent Office. It may be interesting to know that in a patent which I abandoned because I did not think it sufficiently practical there occurred the following passage:—"Another "method is to enclose the armature in insulating material, "leaving a sufficient clearance all over the surfaces of its con-"ductors to enable the oil to run in and entirely cover, or as "nearly cover as possible, all the conductors." Then it says: "It is sometimes desirable to put tubes or passages from the outside "of the oil case to the centre of the oil case." It there refers to the method of circulating the oil and conveying away the heat which in a large machine it would be otherwise difficult to deal with. Notwithstanding my having gone so far in that matter, I let it drop, and, I think, wisely; and it may be worth the consideration of Professor Forbes and the engineers of the Cataract Construction Company whether they do not do so likewise. I have had the pleasure lately of going round some of the works

Mr. Ferranti.

on the Continent, much as Professor Forbes and other engineers of the Construction Company have done. I went, it is true, with a different object, but nevertheless I saw much of what they saw. I recognise in the design of Professor Forbes's dynamo several features of merit which were shown in the last designs sent in to the company by Mr. C. E. L. Brown, of Baden, who is no doubt the man who has done most towards high-pressure work and long-distance transmission. The first point is the winding of the armature wire in slots in the iron; and the second is the method of winding the magnets with flat strip insulated with mica, and so arranged that the coils and insulation would not crush under the centrifugal force to which they would be subjected. I next come to the cooling of the bearings, and to the very ingenious device which I have no doubt, if it has not been already proposed, has been thought out by many engineers with regard to an alarm for heating. I think it says in Professor Forbes's paper that when the contact is made water can be turned on to cool the bearings. I think water circulation is very generally worked among English engineers, and also those on the Continent; and I have also used it myself where very heavy bearings had to be employed, circulating water continually in the jacket round the bearings. But why cannot the water be left running continually, instead of being turned on when anything occurs? Surely there should be enough water at Niagara to make this possible. I regret to say that, with my limited knowledge of the subject (because everyone's knowledge is but regarding the small portion which one has oneself carried out), I consider the idea of exciting a large plant like this in series to be anything but the best arrangement. I believe the old parallel method of exciting has many points in its favour. With regard to the question of the dynamos which are being built by the Westinghouse Company for the Niagara Construction Company, I am rather surprised to see that, after all that has been done with high-tension work—the failures which have been made, and the successes which have been achieved—the Americans have not thought fit to go in for more than 2,000 volts. The question of transforming up is one which, in practice,

is an exceedingly awkward one; it leads to many objectionable results, and it is specially bad where it is combined with other dynamos working direct upon the circuit at high pressure. I believe it is contemplated later on to make these dynamos of high pressure—namely, 20,000 volts—and then I suppose the old ones will either have to be re-wound, re-modelled, or worked on this objectionable plan of transformer and direct dynamos coupled in parallel. There is, I think, a very great difference, which may not appear at first sight, between designing dynamos, and building them as a regular practice—in fact, between a thing which one makes one's ordinary daily business, and getting together a collection of all the information which is available in the world, and then without any very great previous knowledge of dynamo designing, and more especially building, and I may say still more, running, to design a machine which is supposed to fulfil great expectations, and which I am sure everyone would hope might do so. This is the point of, perhaps, the greatest weakness in the Construction Company's scheme. If the designs of any one of the many constructors had been taken, and it had then been left more to them to re-design and build the dynamos, and not to contemplate the idea of designing dynamos for themselves, I should say that that was a surer way of obtaining a successful result. With regard to the next point, the electro-motive force to be used: I understand it is proposed to use a force of 20,000 volts. This is taken rather on the assumption that at Deptford they have been working for the last three years a pressure of 10,000 volts between one conductor and the earth, or between the inner concentric cable and the outer, and therefore it is argued that if both the conductors are insulated the risk of failure will be no greater with 20,000 than with 10,000. I think this is a great fallacy. If you have two cables, both insulated from earth and charged to a high electrical pressure, in this way, the system is exceedingly unstable. I think it is an undesirable plan. If you adopt the safest method of conveying electricity—namely, by concentric cables—then the outer conductor naturally takes the same potential as that of the earth. If the two cables are separate you are liable by any

Mr. Ferranti.

Mr. Ferranti. accident which may occur in practice to have one of these put to earth, and then you immediately have a sort of reversal of strain, and the whole theory of the 20,000 volts being as easy as the 10,000 is upset, and you have conditions which you have not provided for, and which may prove disastrous. I think, therefore, it is rather a delusion to imagine that the safety of 20,000 volts with both conductors insulated is as great as that of 10,000 volts with the outside conductor to earth at one place. The question of the electro-motive force is, of course, a very vital one. I have no doubt that neither the author nor those who are assisting him, especially on the other side of the Atlantic, limit their ideas to 20,000 volts. They do not wish, perhaps, to talk of more, for fear of being criticised and thought ridiculous; neither, perhaps, would they speak of more than 20,000 for the reason that they would rather see how that works first, and I think this is very wise. It is a very fair electro-motive force for a beginner's first attempt. Taking this pressure of 20,000 volts, the question is how to insulate it. The author says that "a subway has been "partly built for carrying the cables," and we have drawings of it before us on the screen. The first impression, and also the last, created upon my mind is that the subway would be what every-one who has been very close to a high electro-motive force would call a very hot place. No doubt many engineers present have been very close to a very high electro-motive force, and have seen demonstrations of this force when there are big dynamos at the other end, with a small self-induction, that can send a very big rush of current to any point of failure. Ten thousand volts is quite alarming, but I think that 20,000, with a greater H.P., would be worse. It is quite possible, if any fault did occur in the subway, that it might make a very serious state of affairs in the whole of the subway, or, at any rate, in a very considerable portion of it, and possibly lead to complete breakdown of the entire system. I should say that Professor Forbes's subway was perhaps the most unsatisfactory method of dealing with the question, and in my own mind I am quite sure that it was only agreed to by the experts on the other side for its having the redeeming feature of being suitable for cable-laying at a not very distant date.

Mr. Ferranti.

With regard to the question of periodicity, it has been spoken of in a very light way by numbers discussing it before me. In the first place, I cannot agree with periodicity of $16\frac{2}{3}$ periods per second, or even of 25. I am sure a considerable amount of lighting will be done, and the most practical way of doing this lighting is direct by transformation from the high-pressure mains. Why, therefore, should this lighting, which must be a large portion of the business, and one for which the current is to be sold at a higher rate, be sacrificed for low periodicity? Is there going to be a proportionate gain in general working of the system, and in the cost of using it? I do not think there will be. I think the periodicity adopted by Ganz & Co., of Buda-Pesth—namely, somewhere about 40—is a more satisfactory one. It certainly renders both arc and incandescent lighting possible, and I think it is somewhere about the happy medium. With regard to the higher periodicity, which we have heard discussed as just as good, enormous difficulties come in. It is spoken of as if it was really not an important thing; but there are very serious effects. In the first place, the capacity effects are very serious, and what is even much worse than that is the magnetic retardation due to the field caused round the conductors in air. But, as I believe this point will be more fully referred to in a paper which will shortly be read to the Institution, I will not touch further upon it, except to say that I feel that it is a very grave difficulty. I have worked in this direction myself, some years before it was generally recognised; but it will be much better left to the gentleman who will put before you the whole question.

With regard to using so high a periodicity as 100, or even, as at Deptford, of 86, it would be practically fatal to an economical working of the system. We now come to a most interesting question—that is, How is electricity going to be transmitted to a very great distance? Is it to be by alternating current or by continuous? I must say I do not for the moment see the faintest chance of doing it by continuous currents with our present methods, but at the same time I cannot conceal from you the fact that I see very great difficulties in doing it by alternating current.

Mr. Ferranti. I hope some day the Cataract Construction Corporation will be able to take electricity as far as New York, 500 miles, and probably to Chicago, 500 miles in an opposite direction; and I do not see why carrying the electricity in this way should not be commercial if it can be done sufficiently well. The question is, What system should such a thing be done upon? It is a matter which will require the very gravest consideration of all the experts of the company.

I have already trespassed too much upon your time, and so I must leave this most interesting subject to turn to a question which I think is much more important to this Institution than the *periodicity* or *electro-motive force* or the design of the dynamo, namely, the way in which the Cataract Construction Company has obtained its information and valuable designs. In the first place, a long time ago, at the period when I, together with two other gentlemen, obtained the concession for using the Canada side falls, the people who owned the rights on the American side could not get the capital together to make a start. It was a question of how they should get the money, for you must remember that there were not then the great capitalists in it that there are to-day. They therefore proceeded in what I call an exceedingly clever way, and we must all admire them for this, though we may not like it. They formed a commission; they asked the greatest scientists of the day to join it; they got a room in South Kensington from the City Guilds, which they made their headquarters. They gave that commission power to offer prizes (very miserable ones), and to adopt any means by which they could collect information. As I have said, I had the pleasure of meeting some of the people who were drawn by this. I myself was not. Various schemes were proposed. These great scientists—most honourable men, whom we would not question in any way—sat upon this commission. They gave it a perfect legality; they gave it the support of their great names. Everybody thought it was the right thing to send in schemes and try to carry off the prizes. It was not done by outsiders, but by people who had to make their living by building dynamos and machinery. When these projects were sent in, nothing was found sufficiently

satisfactory. I know one firm who spent at least £1,000 on doing it, which may give you an idea of the immense work put upon it; but you may take it that that money did not really represent the value of the ability of those men who had worked for years to arrive at that degree of proficiency. What was the next stage? The engineers of the company go round to different constructors of Europe, where information is to be obtained, knowing full well all the time that the information was not going to be really used to the advantage of those who gave it, or adequately remunerated. Mr. Ferranti.

Professor FORBES: You have no right to say that Mr. Ferranti.

Mr. FERRANTI: I have the absolute proof of it in a letter which I hold in my hands, and which was written from New York in July, 1892. I would have read you this letter, which I received from a very important official of the Cataract Construction Corporation, which clearly shows the intentions they had, and that the information sent in would either prove to be public property or was already covered by American patents. They would therefore have to reckon with those in America who held the patents, and not with the people who, after their years of work, should send in valuable working designs. It is upon the plans so obtained that these designs have been got out; it is upon the work of the electrical world, unrecompensed, that the undertaking will be carried out at Niagara. I do not think myself that it will conduce to a good result, and I feel sure that if it had been put into the hands of one constructor—like Mr Brown, for example, who has already experienced what it is to build this class of work, but upon a lesser size—the result would be better and cheaper. I regret to say that I do not think that this Institution can be congratulated upon having had one of its members selected to carry out this sort of undertaking. I do not attach the slightest blame to Professor Forbes personally. I am sure no one would think that I did so in saying what I have; but still the scheme has been most beautifully worked out by the Cataract Company, and everybody has fallen into the trap. We can only regret the result.

Mr. Siemens.

Mr. Siemens: I think it is only right to the commission who asked for these tenders of various people that I should put the matter from another point of view. Mr. Ferranti has told you himself that he did not compete at the time, and therefore perhaps he does not know the conditions under which the tenders were given. I had, at that time, the pleasure of speaking with Mr. Adams, the president of the commission, and he explained to me that the conditions were to be that the various competitors were to hand in detailed designs, and were to supervise the construction of the machinery in America for a commission of 2½ per cent.; and I explained to him that it was quite out of the question to suppose that a commission of 2½ per cent., or perhaps 5 per cent.—I forget which—was an adequate recompense, and that under the circumstances we could not compete, but could simply give a general design without the details of construction. Under these circumstances we did compete, but we did not send in detailed designs, and as a consequence we were excluded from the prizes; but I am perfectly certain that anybody who read the conditions under which they tendered knew from the beginning that it was the intention to construct the whole of the machinery in America. You all know the high duty and the cost of freight for such heavy machinery, and therefore it is only natural that it should be so. It was perfectly plainly stated in the conditions of tender, and it was also stated what the prizes were to be—£500 and £200, or something like that—which I quite agree with Mr. Ferranti was not an adequate compensation; but surely the tenderers knew from the beginning that the company wished to use all the drawings that were handed in as their property, and that if any such drawings were adopted they were to pay 2½ per cent. or 5 per cent. on the actual value of the plant constructed according to those drawings. Therefore I think everybody who tendered, tendered with open eyes, and I do not think that any question ought to be raised now.

Now, as to sending a current of 20,000 volts through underground cables, I would remind you that there was a concentric cable which transmitted alternating current with a difference of potential of 24,000 volts between the Frankfort

main Exhibition and the Marine Exhibition. It was working all the time, and there was no difficulty about it. We have since been using the same cable with 50,000 volts, occasionally going up to 60,000, and there has never been any difficulty. Therefore, I for one would be perfectly ready to supply Professor Forbes with cables that would stand 20,000 volts. Another point which has been insisted upon by a great many speakers, and which Mr. Ferranti mentioned, is that it is quite out of the question that the continuous current could do such transmission. I am not so sure about it. Our own project for the Niagara transmission was by continuous current; and, from the experience we have gained, we are not at all afraid about being able to insulate everything sufficiently well to stand that pressure, because, of course, all motors, and all generators, and everything, ought to be well insulated from earth. In various other projects, not of such magnitude as Niagara, we have had rival estimates made by our Berlin firm, who are great on the rotary current, and our own firm with alternating currents and continuous currents, because we do not care what sort of machine or what current we employ. We wish to employ the best for the purpose. Up till now we have found that in every scheme the use of the continuous current is best, and I am not sure that the Cataract Company will not be converted to the continuous current before it is done. Mr. Siemens.

Mr. EVERSHED: I wish in the few minutes that remain to bring the discussion back from the somewhat dangerous ground on which it was left by Mr. Ferranti. Mr. Mordey referred to the effect of using large solid conductors. Professor Forbes in his paper does not say that he is going to use large solid conductors, and I have no doubt he will not do so. He shows in the diagram of his subway something which may be taken to be laminated strip conductors, so that the skin effect to which Lord Kelvin drew attention will not have any prejudicial effect on distribution. But there is one effect which will come in, and which is of vastly more importance in almost every aspect of the case, viz., the induction between the conductors, whether they be solid or laminated. It is quite easy to work out what the back electro-motive force due to this induction will be per mile for Mr. Evershed.

Mr. Evershed.

any pair of conductors running at any given distance apart, and I have worked out a few cases, taking the conductors to be 6 inches apart, or 12 inches or 3 feet apart. I will go on the best assumption for Professor Forbes, namely, that the conductors are 6 inches apart. I also take the density as 333 amperes per square inch, which I understand he proposes to use. I find there is one great argument in favour of the use of as low a frequency as possible, in the enormous inductive electro-motive force he would have in his line if he were to run at the frequency suggested by Mr. Mordey, viz., 100 periods. I fully agree in thinking that 100 is by far the best, if you can get over that difficulty, but it is a very serious one. If you have mains 3 square inches in section, such as Professor Forbes would have to use in the pioneer plant at 2,000 volts, and place these 6 inches apart, and work at 100 periods per second, you will have an E.M.F. of 880 volts per mile of conduit from induction alone. The drop from resistance would be only 27 volts per mile, so that it is absolutely nothing as compared with the inductive E.M.F. Working at 25 periods, which I understand Professor Forbes intends to do for a time, the inductive E.M.F. per mile is reduced to 192 volts, while the resistance drop will be 27 volts, as before. On that ground alone it appears to me that Professor Forbes has an almost unanswerable argument in favour of the use of low frequency in this particular case. The only other point that I desire to refer to is the question of coupling the alternators in parallel. I want, if possible, to get Professor Forbes to tell us distinctly what are the uses or advantages of artificial load in coupling machines in parallel. It so happens I have been in a great many central stations, and have seen machines of all kinds, for alternating and direct currents, and of the largest size, put in parallel, and in England I have never seen an artificial load used. The ordinary and common-sense practice is to run up the machine that is to be put on to the omnibus bars to the same potential as the bars. The switch is then closed, and nothing happens. The speed, or excitation, is then increased until the machine takes its proper share of the load. I understand that on the Continent the machine is first

of all carefully nursed up with an artificial load until it is running on the load which it should take if it does its fair share when on the omnibus bars. The object appears to be to prevent shock to the whole station, but it is obvious that the machines that are already running receive a shock (a negative shock, if you will), viz., that their load is largely and suddenly reduced. It would be very interesting if Professor Forbes would tell us, not what Messrs. Ganz think or Mr. Brown thinks, or the Westinghouse Company find in their practice, but what he himself regards as the main advantage of the use of artificial loads in coupling large machines in parallel. Mr. Evershed.

Mr. CROMPTON: I will not take up time by congratulating Professor Forbes; we all congratulate him on the fact that a member of this Institution, and an English engineer, has been chosen for a post of such honour and importance, and we know that he will fulfil his duties thoroughly and conscientiously; but there is one important omission from his paper which has not been sufficiently touched upon in the discussion which I now notice, and that is, that he has been so guarded in introducing financial questions into his paper that it affords us little or no information on what is, after all, the greatest interest to us here, viz., the ultimate cost to the users of power transmitted to considerable distances, as compared with the cost of such power generated by themselves on the spot; in other words, if we had been able to gather from the paper what would be the prices charged to the users at Buffalo, or the more distant points intended to be served, we should have been better able to appreciate the special features of the problem which the paper is doubtless intended to bring before us. Mr. Crompton

I, in common with, probably, other engineers now in this room, have often had before me schemes for transmission of power for which I have had to prepare estimates showing at what price the power could be delivered to consumers so as to yield a profit to those who financed the scheme; but I have in many cases been disappointed, as I have been unable to show that power thus transmitted could be sold as cheaply as it could be generated on the spot in the ordinary manner. We all know

Mr. Crompton. that the Lauffen-Frankfort transmission experiment proved nothing in this respect. To me it appears that to discuss this Niagara transmission of power project without telling us anything about the cost, is like playing *Hamlet* with Hamlet's part left out.

I have been forestalled by Mr. Ferranti in some remarks that I was about to make on the subject of the way in which the brains of European engineers had been picked by the Cataract Construction Company; but I must confess that the Americans have only done on a large scale what many of our own engineers have been doing every day for some years past—that is, appropriating ideas and details from the older manufacturers. I speak feelingly as a member of a firm which has spent much "blood "and treasure" in fighting the battle of electrical engineering development, and find, in common with many other workers, that the patent laws have been quite unable to protect us or to secure to us the fair enjoyment of the reward of our labours.

With regard to the use of alternating or continuous currents for this transmission, I am inclined to agree with Mr. Siemens. I do not think that the continuous-current transmission has had a fair show in this matter, as Professor Forbes tells us it is on his *ipse dixit* that the alternating current was introduced, after a very hard fight, and in opposition to the wishes of the American engineers. This I can readily understand, as there is no doubt that in America they have had far more experience in dealing with continuous currents of high E.M.F. up to 5,000 volts than they have had with alternating currents of the same E.M.F. I think the grounds on which Professor Forbes stated he condemned continuous currents are wholly insufficient: that the insulation of several armatures arranged in series could be easily dealt with; and, if necessary, it would be just as easy a matter to deal with the insulation and commutator question for direct currents at 5,000 volts as to deal with the same questions for alternating currents of the same voltage. Although I have so often spoken of the superior advantages which, up to the present, the continuous current presents for distribution purposes, yet I have never held any special brief for the continuous current for

Mr. Crompton.

transmission of power questions. In fact, in many ways when single machines are employed transmission by alternating currents seems to offer marked advantages; but it is useless for anyone to stand up in this room and state that no difficulties exist in the matter of parallelising alternators. Grave difficulties still exist, and they are well known to those who have already taken part in this discussion. These difficulties may be minimised by Professor Forbes by the low frequency he proposes to adopt; but I think, with Mr. Brush, that he will not get over those difficulties to the fullest extent until he adopts continuous currents. The change of fashion in these questions among a certain class of engineers is very rapid. I have been a pretty steadfast adherent to the superior advantages and simplicity of continuous currents, and I find that many who went after alternating currents are now beginning to come back to my ideas. I have known all along that the great firm of Siemens & Halske, on the Continent of Europe, were of my way of thinking, and I am glad to hear Alexander Siemens join his testimony to mine in this matter. In considering these large-scale transmission of power problems, after all, the most important question is the first cost and cost of maintenance of the conductors. A former speaker—Mr. Kapp, I think—has pointed out to you that after all, theoretically as well as practically, for a given section of copper, continuous currents are theoretically the best for transmitting electrical energy at any E.M.F. I say that continuous currents can also be best dealt with practically, and that the insulation of continuous currents of high E.M.F. has not in the past presented anything like the difficulty that has been met with in the case of alternating currents, whether this be due to the resonance effects or to the extreme E.M.F. or not. But we *do* know that continuous-current arc lighting circuits have been run in America at very high E.M.F. for many years without any practical trouble, although the material used for insulation of the conductors was such as would not be considered first-rate in this country.

When Mr. Siemens says that he would have no difficulty in making continuously insulated cables stand 20,000 volts, he omits

Mr. Crompton.

to give us what would be the effect on the total cost of using continuously insulated cables of the sections mentioned by Professor Forbes. I cannot help thinking that the cost of such insulation would be so high as to make the capital expended on the conductors so great that it would be difficult to work the scheme to a profit. Professor Forbes no doubt had this in view when he proposed the use of bare conductors in a culvert. I am fully with him as to the great advantage of the bare copper system when large sections of copper have to be used. As is well known, I have persistently advocated such use, and have been perfecting the details for some years past, with very satisfactory results; but my practical experience in this particular work leads me to doubt very much whether the culvert shown by Professor Forbes is a practicable one. He refuses to inform us whether the conductors of one sign are carried on one side, and those of the other sign on the other side. If he does this for purposes of safety, he falls into the difficulties of retardation effects, and which are of great importance; but it is quite certain that I for one should be very sorry to have to maintain bare copper conductors of opposite sign placed as close to one another as shown on the drawing, as, if when the proposed 20,000 volts is used an arc started at Buffalo, it would not be long before it destroyed the contents of the tunnel the whole way to Niagara.

Has Professor Forbes ever seen a high-tension switch-board under conditions which frequently occur in practice when large arcs are started in more places than one? I think if he had seen this he would be rather chary of proposing the use of bare copper in such close proximity when 20,000 volts are used.

Another point I wish to ask Professor Forbes is, whether he has sufficiently considered what is the insulating power of porcelain insulators which he has shown. I and those working with me have been endeavouring for some years to find out a thoroughly satisfactory material to specify for these insulators even for the comparatively low E.M.F. used by us, viz., 200 to 250 volts. We find that for such purposes the ordinary porcelain or earthenware material that is found in practice quite good enough for aerial lines such as the Post Office telegraphs is unsuitable for

our purposes, as the conditions under which the insulators are worked differ so greatly in the two cases. Mr. Crompton.

The invention of oil insulators was one which was supposed to get us out of our difficulties, and which would enable us to obtain sufficient insulation with ordinary porcelain or glass; but I think there are many in the room who will agree with me that oil insulators, after extended trial, have not given all the expectations that were hoped from them, and that as a whole they had been discarded in favour of carefully designed porcelain insulators made of the right material. I have only to remark, in regard to the author's remarks respecting the periodicity adopted by Messrs. Ganz, and afterwards remarked on by Mr. Ferranti as being satisfactory for lighting purposes, that my opinion differs from these gentlemen. I have frequently taken notice of the arc lighting carried out in various cities by Messrs. Ganz & Co. at 42 periods, and I have invariably found that it was unsatisfactory, fatiguing to the eye, and in no way comparable to arc lighting by continuous currents.

Mr. WEEKES: I should like to add a few remarks to what Mr. Evershed has said respecting the self-induction effect in the mains. At the high voltage of 20,000 volts and frequency of 25 this is negligible for the current developed at present at Niagara. Thus, 18 miles of conductors 5 inches apart when carrying 100 amperes, at a density of 350 amperes per square inch, would only have an E.M.F. due to self-induction of 560, which is in quadrature with the dynamo E.M.F., and therefore the effect negligible. Mr. Weekes.

In the absence of any information from the author as to the relative position of the two circuits in the conduit, I would point out that in the position of highest insulation resistance the mutual induction effects will be serious. If the two circuits carrying the currents differing 90 degrees in phase are arranged with all the leads one side of the conduit, and all the returns on the other, then the inductive effects produced by the one circuit will induce an E.M.F. in the other circuit in phase with that of the machine supplying that circuit. The resultant effect is to raise the voltage along the circuit whose current lags, and

Mr. Weekes. to lower the voltage in the wire carrying the leading current. Assuming four machines to be supplying 100 amperes each to conductors so arranged, at 20,000 volts, the potential difference at the motor end 18 miles away (with the dimension of the Forbes conduit as given) will be, neglecting copper losses, 21,400 volts on the lagging circuit, and 18,600 volts on the leading. This effect is serious, as a transference of power actually takes place. To overcome it, the wires in one circuit should be arranged in a plane at right angles to the plane containing the other circuit. In other words, place the leads from each circuit on the upper insulators, and the returns on the lower insulators.

In these estimates I have added nothing for the increase of the effect due to the iron supports, which may easily double the drop if commercial cast iron is used.

Mr. Mavor. Mr. A. E. Mavor: It is notable that the most severe criticism of Professor Forbes's scheme for carrying high-tension currents along bare conductors in a culvert has come from the principal exponent of the similar method of conducting low-tension currents. During the last year or so I have been carrying out for my company a great deal of work in culverts, and it would be interesting if Professor Forbes could tell us how he proposes to ventilate the subway. I have recently had some experience of the fluctuations of atmospheric conditions in such situations, and the difficulties that will arise in conveying high-tension currents along bare conductors in such a culvert as that described must be very great. The methods of draining and drying the tunnel are quite insufficiently described. The only reference made to the draining is where the author says that at every 400 feet there will be four 3-inch drain pipes, and afterwards these same drain pipes are to be used to convey the branch leads into the tunnel. The drain pipes are also to be closed at the outer ends—I presume to prevent the admission of water; but after the pipes are employed as cable conduits, can they still remain efficient as drains?

Professor Forbes: You have misunderstood that altogether.

Mr. Mavor: With regard to the skin effect, I do not think Professor Forbes has at all clearly demonstrated that a number of

smaller concentric conductors in parallel would not completely overcome the skin effect. Lord Kelvin's objection really does not hold for conductors of small section, and there does not seem to be any reason why such conductors should not be employed put into parallel. The author has said that Mr. Tesla has clearly demonstrated the rapid deterioration of all solid insulators with a high frequency, but he did not mention that Mr. Tesla employed a high voltage; and many of the destructive effects of high frequency and high voltage are also present at low frequency and equally high voltage. Mr Mavor.

General WEBBER: I think we ought all to feel sympathy with Professor Forbes in connection with the paper from one point of view—viz., that it is brought before us as referring to a work which has not yet been carried out; and therefore, in criticising the paper, we should remember that modifications during execution will infallibly have to be made. I hope that the criticism that we have heard on the details will not go forth from this Institution as having been made without the full knowledge that this was well understood by the speakers. We ought not to forget that Professor Forbes told us at the beginning of his paper that it was divided into two parts—first, the plan of the work of which he, as consulting engineer, has recommended the adoption; and, secondly, a description of the design of the machinery, which has yet to be made; also, that he has given due credit, not only to those who have helped him, but to those who have given him advice. He says: "I have been "benefited by the courtesy and the kind assistance of pro- "fessional men, inventors, and manufacturers in both continents." Very few consulting engineers in his position would have ventured to afford us the pleasure and instruction of hearing an account of works which at the present moment are little beyond a state of design, and I submit to the meeting that we ought to feel much gratitude to him for letting us so far into the secrets of this great work in the future. Professor Forbes, in answering those criticisms, will, I hope, recollect that we cannot expect him to answer in as full detail as he might wish the questions which have been put; and I for one shall feel that, General Webber.

General Webber. if he fails to do so, it will be the natural consequence of his position as confidential adviser of the Cataract Company.

Mr. Brown. Mr. C. E. L. Brown [*communicated*]: In reading the paper of Professor Forbes on the electrical transmission of power at Niagara, there is one point which is apparent before all others. The undertaking is a general system of distribution, both of power and light. This being the case, I cannot help thinking it is a mistake to adopt so low a frequency as 25 $\sim$.

It must not be supposed that I am not an advocate of low periodicity, but I understand that it has its own sphere of usefulness, and should not be carried too far. It is, perhaps, not generally known that I have, since 1891, designed a large number of plants (the most important being one of 1,000 H.P. in Russia) with frequencies varying from 15 to 25. In this way I have, of course, attained no little experience in this matter, so that I may judge pretty well of the advantages and disadvantages of a low frequency.

I will now, in a few words, give my views of the good and bad points of high and low frequency.

Generators.—With these it is largely a question of size, speed, and type of machine. For low speeds, in comparison to the size of the machine, a low frequency is evidently the best. Thus, in one of the cases mentioned above, where a 100-H.P. generator had to be designed for a speed of only 30 revolutions, it became necessary to use as low a frequency as 20 $\sim$. Applying this principle to the case of Niagara, where we have a very high speed for the size of the machine, the easiest solution will be attained by adopting a higher frequency.

Regarding ease of working in parallel, I have been unable to detect any difference between very low frequencies, such as 15 $\sim$, and up to 100 $\sim$. It is astonishing that even at the present day so much is still said of the difficulty of running alternators, single or multiphase, in parallel. I have run machines of very different types in parallel under the most different conditions, and have never found the slightest difficulty. Indeed, I have come to the conclusion that the coupling of alternators in parallel is as easy, if not easier, than of direct-current

machines. I may, for instance, mention a case of parallel Mr. Brown. coupling under peculiarly unfavourable conditions. One of my smooth-armature generators of 600 H.P. direct coupled on the vertical shaft of a turbine running at 120 revolutions, with a frequency of 50, having a very small drop of potential, and giving a curve differing almost inappreciably from a true sine curve, runs successfully in parallel with a Ganz alternator of 120 H.P., driven by another turbine by means of toothed gearing at about 250 revolutions, and having the well-known peaked curve and a very great drop of potential. No artificial load is used, and the equalising currents are unimportant. Indeed, I have never found an artificial load to be necessary.

Regarding the skin effect, I think there is no need to be afraid of a high frequency, as the copper conductors for mere structural convenience would never be made so that the skin effect could be harmful. Capacity is the only thing which makes a low periodicity desirable.

Transformers.—Here there is no doubt that a high frequency is best, and as in the Niagara scheme, if carried out as proposed, the cost of transformers will play a very important part, economy in their design must receive due consideration. It must be specially remembered that we have to deal with very large transformers, and those electricians who have had experience in designing such know pretty well that it is far more difficult to design large transformers to run cool than small ones.

Motors.—With regard to motors, they may be constructed, by suitable design, for high frequencies to give results quite as satisfactory as with low. It is only when we get above 60 ∿ that, in order to get a reasonable speed with small motors, a number of poles is required which gives a rather large value to the apparent watts.

Lighting.—Considering the possibility of supplying light at the same time as power, there is abundant practical evidence that for incandescent lighting with the usual 100 volts and lamps, down to 8 C.P., it is advisable to be well above 30 ∿, and for arc lighting not less than 40 ∿.

Mr. Brown. In conclusion, I think that for Niagara a frequency of 40 to 60 ∿ would be most advantageous, as it would—

1st. Suit for the design of the generator.

2nd. Allow the construction of cheaper transformers than with 25 ∿.

3rd. For the motors it is just the frequency most convenient in every respect.

4th. It allows the feeding of incandescent and arc lamps direct, without costly transformation devices, which is a great convenience.

5th. The effects of capacity, to judge from experience in analogous cases, have not to be feared.

To adopt the two-phase system, using four wires seems to me a happy arrangement, and, to judge from my own experience, I am sure none but satisfactory results can be obtained. This is not changed by the fact that quite practical single-phase motors are on the market giving very satisfactory results. Even small motors, with relatively low speeds, show quite a good efficiency. For example, I may mention that my normal 1-H.P. motor, running at only 1,200 revolutions per minute, has a commercial efficiency of 75 per cent., which, by further change in detail, can easily be augmented to 80 per cent.

It certainly seems remarkable, considering the number of designs sent in, "many of which," he says, "were extremely "good," that Professor Forbes should have decided on what seems to me about the worst arrangement possible for his machine. I cannot imagine a more inconvenient arrangement than the magnet wheel revolving outside the armature, so that it is impossible to get at the machine while in motion, besides losing almost entirely the ventilating effect of the revolving field for cooling the armature. Professor Forbes gives, as his principal reason for adopting this arrangement, the difficulty of holding the field coils against centrifugal force. I can only say that there exist half a dozen means of holding the coils, all giving as much security as the outside ring of Professor Forbes's arrangement. As the field magnets may be made of a single steel

casting giving ample strength, the additional pull due to magnetic attraction is of no importance. Mr. Brown.

Regarding strains on the bearings, I cannot see, if the field be properly balanced, that there can be any to speak of; and practical experience confirms this view, as in the many generators with vertical shafts which I have carried out I have never had any heating.

Professor Forbes.

Professor FORBES (in reply): I have to congratulate you on the fact that this evening's discussion has somewhat redeemed the character of the Institution. There has been shown this evening more of that capability of dealing with a problem of this importance than was shown on the last occasion; because I know that on the last occasion many who had studied this problem to a great extent, and who knew its difficulties and pitfalls, felt that some of the speakers had not begun to grasp the character of the problem which we had to deal with. I felt nothing but sorrow for the position of this Institution in having such speeches made on this important problem. But I must say that to-night there has been more appreciation shown of the difficulties and of the character of the work done. I willingly find an excuse for some of those who have addressed us, seeing that they had had a very short time to consider the question since the paper was delivered, and it was not to be expected that a problem like this could be grasped after the work of a few days or a few evenings spent upon it. There have been some criticism, some hard words, and strong opinions expressed, sometimes injudiciously, sometimes apparently after considerable thought; but nearly every adverse view to those that I have adopted has been combated by other speakers. I feel that in the discordance of the views which have been expressed by the various speakers I see a reflection of the train of reasoning which has been going on in my mind during the years I have devoted to this subject, while I have been weighing the *pros* and *cons* of each course of action.

Now, before I refer to the criticisms, there are one or two general points that I may deal with. With regard to the question of low frequency, I must say that the manner in which it

Professor Forbes.

has been received has been perfectly astonishing to me. I did not claim to myself one atom of credit for having, after a most careful consideration, arrived at the conclusion that the lower the frequency we can get into the design of our dynamo, the better was it for the work. But now, after what I have heard, I look upon myself almost as if I were the discoverer of low frequency. There is a very great divergence of opinion, and I feel confident that such gentlemen as Mr. Kapp, Mr. Mordey, Mr. Ferranti, and Mr. Swinburne, who expressed opinions against this low frequency, will as time goes on arrive at a different conclusion. If I had the slightest personal feeling in this matter, wishing to gain credit to myself rather than success in my work, I should enjoy these objections raised by men so distinguished in their profession. I should look forward with pleasure to the time when I could say, "This system, which is working well at Niagara, was "the system which was found to be faulty by Mr. Mordey, Mr. "Kapp, Mr. Ferranti, and Mr. Swinburne." I should then be enjoying, if such were in the least in my mind—which it is not—a sort of triumph which has been thrust upon me with regard to the question of alternating currents. In 1890 I presented to the International Commission a report on the manner in which the work ought to be done, and insisted that it should be done by alternating currents. The members of the commission were every one of them opposed to it. Since that time all of these commissioners except one have come round to the view that alternating currents are the only ones to adopt for our purpose, and I naturally feel proud of having insisted upon this at a comparatively early period.

I will say a few words now on the subject brought forward by Mr. Ferranti, and which has been also alluded to in the technical Press. It has been said that the International Commission was a farce, and that those who prepared the schemes were treated unfairly. Mr. Siemens has given a complete answer to this. It has also been said that it was determined to give the men who furnished the plans which should be adopted the superintendence of the work. Gentlemen, I hold in my hand the report which I submitted at that time, in which I said that

Professor Forbes.

the alternating current was to be used; that it was to be of two phases; that 2,000 volts were to be used for the local work; that step-up transformers were to be used to carry it to Buffalo; that the motors were to be synchronising motors, Tesla two-phase motors, and motors with laminated fields and commutators. The Cataract Construction Company have been absolutely loyal to their compact in giving me the superintendence of this work. In that report I gave estimates and drawings of everything; and the only mistake I made was to recommend the adoption of the Mordey alternators. We know now that they are not of the right frequency, and we know that in certain other points they would not suit our purpose.

Dr. Fleming's remarks were the redeeming feature of the discussion on the last occasion. I must say that he showed himself to be a practical engineer in the highest sense of the word—that is, a man of great experience in the line which he was discussing, and who knows how to apply theoretical considerations to the solution of practical problems. If the discussion has been somewhat barren in some parts, we have at least received from him an account of the method employed at Deptford for overcoming the effects of electrical resonance; and that is a very important point. The endorsement he has given to my own views I consider to be of great value, because we know that he has given some attention to the subject. When he says that "the lines should never in practice be suddenly connected "or disconnected from the machines," he said something which other speakers considered was an axiom. I may say it may be the practice—and I am glad to say it is largely the practice in this country—but it is absolutely contrary to the practice in America. When he says "the advantage of adopting a low "frequency and low capacity is something enormous,"—and when he says "there can be no doubt about the wisdom of selecting "this low frequency for a case such as the Niagara transmission, "of which the question of the utilisation of the power by motors "is the important matter, in which there are enormous advan- "tages to be gained by the reduction of all these capacity effects, "by the employment of a reduced frequency,"—we recognise that

Professor Forbes.

grasp of the special features, and the principal dangers to be avoided, that were so markedly absent from most speeches.

I will now go on to Mr. Mordey,* and will give him a little more time, as it might be discourteous if I did not answer the points that he brought forward. I must say he failed entirely to grasp the situation. Accustomed as he is to deal only with electric lighting plants, and those which we may, without discourtesy, call comparatively small plants, relatively to that we are now dealing with, he does not realise what our plant is to be, and that it is essentially a *power*-distributing plant, and that the electric lighting which is to be done is utterly insignificant compared with the rest of the work. His opinion that I have thrown down the gauntlet to electric lighting men shows that he has not realised the position of affairs. These remarks must be taken also as an answer to Mr. Ferranti and others who have complained of the neglect I have shown of electric lighting; but you will find in the paper that, while I have provided all means for dealing with such electric lighting as we shall have to do with, I look forward, almost with certainty, to the practical development of transforming current-rectifiers, which will render the electric lighting system as economical and satisfactory as any which could be devised.

The discussion of efficiencies of small transformers of a few horse-power, as being applicable to our case, shows Mr. Mordey's want of appreciation of the problem. His willingness to condemn the artificial load on the ground of expense also shows it.

With regard to parallel working, he says, "If 100 periods "succeeds, why go to 25 periods?" I think I have shown a great many reasons for going down below that. It is perfectly true that 100 periods will allow machines to work in parallel, so as to run without getting out of step; but that is not all we want. We want them to run in step without a waste current passing from one machine to the other, and without breakdowns. I do not consider that 100 periods has succeeded so well as he would lead us to believe. He knows perfectly well that his alternator, although it works in parallel,

* My reply has reference generally to the shorthand notes of the speeches. This will explain any discrepancies with the printed discussion.—G.F.

Professor Forbes.

does not work perfectly; he knows that his machine has breakdowns at times. We, at Niagara, cannot afford to undergo such an experience as he has met with in his comparatively small machines. Even supposing that it was not the high frequency which caused the breakdowns, then it must be the absence of an artificial load, or else the fault lies partly in the design of the machine. As consulting engineer to an important work, I cannot take any of these risks, whatever the cause of failure may be. Mr. Mordey's high-frequency alternator does not work well enough for our purposes. At the time when I recommended the Mordey machine I said that, before deciding to adopt it, tests must be made with a resistance between the dynamos in parallel. Does Mr. Mordey forget that, when he kindly placed at my disposal the means of making this test, by putting "sunbeam" lamps in series—that is to say, between the two dynamos working in parallel—the lamps went up and down in brightness with a periodicity of a few seconds, and that the tendency was to rise to maximum brilliancy, showing that the dynamos were rather working in series than in parallel? From the moment of those experiments, I felt that that type of machine was not suited for our purposes, though I never mentioned that until I was compelled to by Mr. Mordey's claims for his special mode of working.

With regard to artificial loads, Mr. Mordey has said that he thought it would introduce extra dangers. I have been utterly unable to see what those dangers are; otherwise, the only objection that has been raised to them is that they are somewhat of a complication. When you put an unloaded dynamo on to a full load, there is a considerable drop, which may be accompanied by electric resonance; and all that I claim for the artificial load is that it is a help to secure us from breakdowns. It was found essential in the Tivoli installation; and, as we cannot afford to have breakdowns, I look to every safeguard that can possibly be devised, and wish to introduce every possible one into our work, even though the expense be somewhat greater. In this case there is no additional expense, because the artificial load, when not required for that purpose, can be

Professor Forbes.

used for a useful purpose, which I need not dwell upon now, but which has been fully considered in my plans.

Mr. Mordey's remarks about skin resistance would be accurate if you could subdivide the 36 square inches required for the conductors of our first 15,000 H.P., if they worked at 2,000 volts; but if he had grasped the problem, he would see that you cannot get space for the enormous collection of conductors unless you mass them together, and that, at high frequency, skin resistance then comes in. His remarks on transformers are those that caused me most regret, as showing how far he is from grasping the problem. He does not appear even to appreciate the work that must be done by consulting engineers, especially when responsible for so important a work as that on which I am engaged. An engineer would be very little likely to advise a scheme such as this until he had every detail worked out—at any rate, to a very large extent. Foreseeing difficulties with manufacturers who might hold views like Mr. Mordey, I set to work to design transformers of low frequency for every kind of purpose for which we shall want them. Designing transformers for special cases, and for larger sizes than usual, is somewhat laborious work, but the results are quite certain when we know the quality of the iron used. The three things that want attention most in our case are cost, cooling, and efficiency; and the last is almost of no account when dealing with the large transformers required for our work, very little work being required with transformers under several hundred horse-power. In these cases there is no difficulty in getting efficiencies much higher than those mentioned by Mr. Mordey; and, even if a half per cent. could possibly be gained by increasing the frequency, I would infinitely prefer to retain the advantages of low frequency. Mr. Mordey says he places the motors in the list of things that do not supply any argument in favour of the reduction of frequency. He has not given us a particle of evidence to support this view, whereas I have given experimental proofs and convincing theoretical explanations. On the question of the electro-motive force of the dynamos he is completely at sea. He wants us to use 500 volts, and I suppose he would use that pressure for the local work. If he will kindly

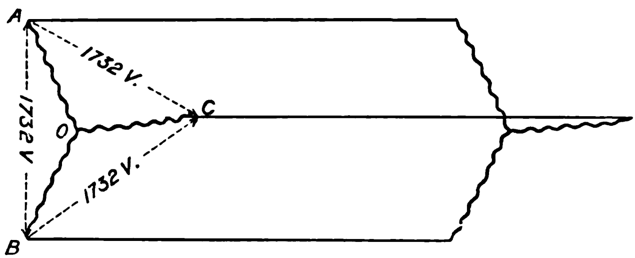
estimate the section of copper for 50,000 H.P. at this pressure, he will see the difficulties to be met with. The copper section is about 400 square inches, and I have no doubt he has never realised the fact until this moment. I can quite understand that, with his experience of one type of machine, he would not go above 2,000 volts; but I am surprised that he is not able to realise these two facts—first, that with a 5,000-H.P. alternator the amount of space taken up by insulation, compared with the size of the machine, is quite insignificant compared with that in smaller sized machines; and, in the second place, that if you shirk the difficulty of insulating the 20,000 volts in the fixed armature on the dynamo, you have to face the difficulty when you come to the fixed transformer, and he has not shown us where the difficulty lies. Mr. Mordey's remarks on lighting are quite beside the mark. Ours is *not* a *lighting* station. Mr. Mordey hopes that I shall have a good deal to say about capacity after a year or two's work. I earnestly trust that I shall have as little as possible to tell you about capacity after a year or two's work. One of the main objects of my design has been to get rid of capacity. I notice that Mr. Mordey has, in the correction of the shorthand notes, erased his statement that the selection of low frequency prevents us from getting lights without flickering by the use of rectifying machines. It is well that he has corrected his mistake. Professor Forbes.

Mr. Kapp deals with the relative economy of conductors with direct current, single alternating, two-phase three or four wires, and three-phase currents. He has not put his calculations before us; but I received from Professor Elihu Thomson his views, which Mr. Kapp says he supports, and I will now insert these views as stated by him, with the modification which I find necessary:—

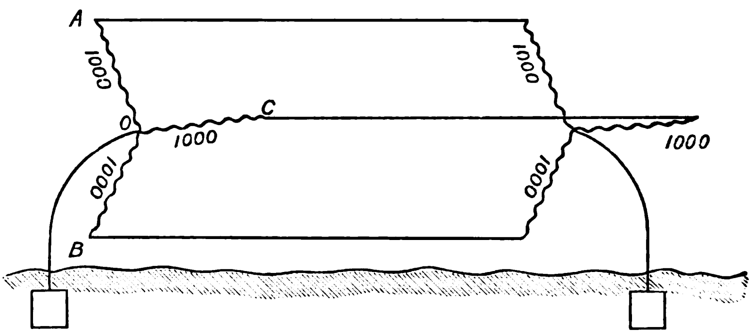
" It is only necessary to observe that if A, B, C be the three-
" branch circuits of a triphase generator whose voltage between
" the adjacent terminals is maintained at 1,732 effective volts,
" the E.M.F. in each branch, measured to the point O, or common
" connection, is 1,000 volts effective ($1,732 \div \sqrt{3}$). Apart
" from arithmetic, it is evident from symmetry that, with

Professor Forbes.

" balanced loads and uniformity throughout the triple system, " the point O is the electrical centre of the arrangement, and



" its potential will always be zero, so that we can ground this " point without altering the distribution in any way.



" Grounding this neutral point at both ends of the line, it " can be shown arithmetically what might be expected from the " symmetry—that the current through the ground wires from the " A branch, say, is just equal and opposite to the current from " the ground to the B and C branches together. So that no " current flows to the ground so long as balance in loads is " maintained, but evidently each of the three circuits may be " regarded as a separate circuit worked with ground return at a " pressure of 1,000 volts—a condition which is equivalent, in " regard to cost of copper, to loop transmission on 2,000 volts,— " just as in the three-wire system with grounded neutral."

(At this point the arguments adduced are erroneous, assuming that the copper required is inversely proportional to the square of the E.M.F. The continuation of the argument should be as follows:—)

Professor Forbes.

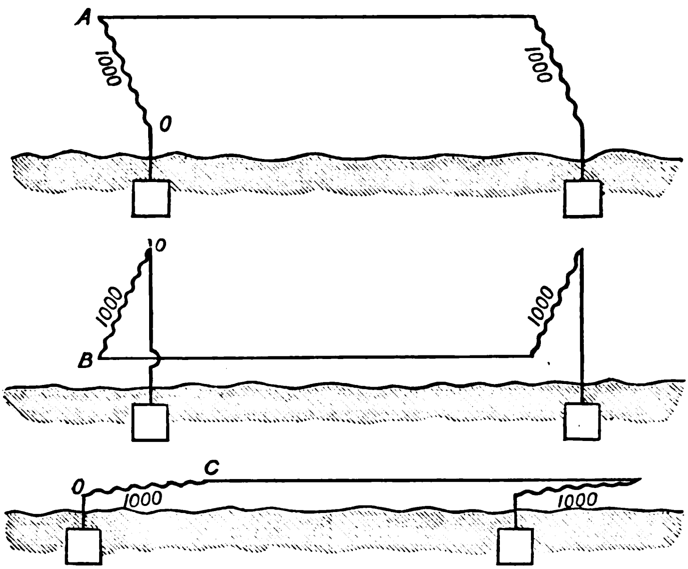
Hence, if we consider the lag to be the same in both two and three phases, we have—

$$\frac{\text{Total out-going current on three-phase}}{\text{Total out-going current on two-phase}} = \frac{1{,}732}{1{,}000};$$

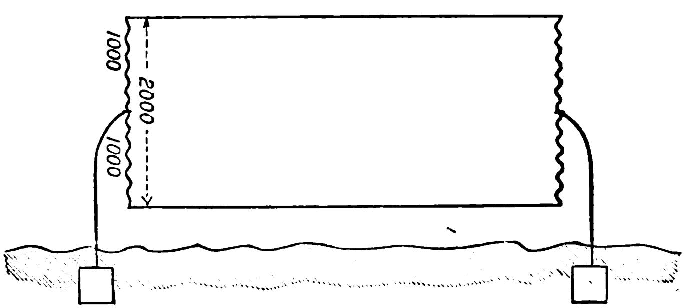
and

$$\frac{\text{Total copper on three-phase}}{\text{Total copper out and return on two-phase}} = \frac{1{,}732}{2{,}000} = \frac{87}{100};$$

or 13 per cent. less, in favour of three-phase.



(The amendment which I have made in the argument is, that I have adopted Lord Kelvin's rule of the economical section, in which the amount of copper is in proportion to the current.)



If, however, we use a common return for the two circuits of

Professor Forbes.

the two-phase system, we save, as correctly stated by Professor Elihu Thomson, "60 per cent. of one of the four conductors "previously needed, or 15 per cent. of the previous total." Thus we have the following proportions of copper required for the three methods:—

Two-phase, three wires, section of copper		=	85.
Three-phase, three wires,	„ „	=	87.
Two-phase, four wires,	„ „	=	100.

Mr. Kapp has tried to prove me wrong when, at the end of my paper, I prove that the losses in the field of a multiphase motor are increased 42 per cent. when we double the frequency. He admits the 42 per cent., but says that the machine with twice the number of poles, and running at the same speed, gives 2½ times the power. If it was not Mr. Kapp who had said this, I should not have taken any notice of it whatever. If Mr. Kapp will look at the diagrams at the end of my paper, he will see that the same total number of lines of force are cutting the wires of the armature at the same rate, and therefore give the same E.M.F. and current, and the same power. There is no doubt whatever of the power generated in these two machines being equal, and the 42 per cent. remains as I gave it. Mr. Kapp says you cannot find space to put rings on a direct-current motor to convert it into a synchroniser in the manner which I have described. I can only say that I did this last summer with one of the stock machines supplied to us by the Westinghouse Company. Mr. Kapp says he does not know how he should start such a motor, although that motor would only require to be started once in many months. I have done such a thing without having the necessity of using Tesla motors, and I shall be very glad to show it later on when the thing is running at Niagara. Besides, since we have two phases, we can put on four rings, and so make the motor self-starting. I have not the slightest fear of our being troubled. Mr. Kapp says he has objections to low frequency with motors, but he does not tell us his reasons. I must thank Mr. Kapp for the way in which he speaks of the machine shown in the drawings and described in my paper. He says: "I have studied the design of Professor Forbes's

"alternator very carefully, and I may say at once that the "machine appears to me to be of a very excellent type." I am extremely glad that a man of such eminence in design as Mr. Kapp is able to speak in that way of this machine. Professor Forbes.

Mr. Ferranti has been good enough to tell you that I have stolen this machine from Mr. C. E. L. Brown. Mr. Ferranti has told you that I have been travelling over Europe, looking at what manufacturers are doing, and asking them for designs, while the company knew all the time that the designs offered would not be accepted. I need hardly say that, if I dreamt or believed that such a thing was being done,—if I saw now that the Cataract Construction Company were acting in any unfair way like that,—I should cease to be connected with the work. With reference to this unwarranted attack, I will simply make four statements:—

(1.) With the design sent in by Mr. Brown there were two prices—one for the machine made in Switzerland; another for working drawings, with the object of manufacturing in America.

(2.) If the design of that gentleman, or any other manufacturer, had been thoroughly suitable for our case, that design would have been accepted, and the price quoted paid for it, whether it was made in America or Europe.

(3.) The Niagara enterprise is probably the first American enterprise that ever came to Europe, saying, "We have already "subscribed our capital in America, and we are ready to exchange "such portion of our cash as may be necessary to secure the "brains to properly conduct our enterprise." I confess I am ashamed of my countrymen, and cannot imagine what is the animus which has led to so wilful a misconstruction of such liberality, foresight, and straightforward dealing.

(4.) It has been generally understood that no private undertaking has paid so much for engineering advice, in advance of contracting, as the Niagara project. Most of these disbursements have been made to English, Swiss, and French engineers, and they amount already to about 90,000 dollars.

Mr. Ferranti, however, has said that my design is taken from that of Mr. Brown. Mr. Brown says it is the worst possible

Professor Forbes.

design that could have been devised. I was sure that it was simply a ridiculous assertion that the design of that machine could be said to be in any sense like that of Mr. Brown, and I am glad to be able so promptly to bring a quotation from Mr. Brown to show that he does not think so either. Mr. Ferranti has, however, brought a specific charge, by saying that I have taken from Mr. Brown's design the idea of winding the armature with copper strip insulated with mica. Is it claimed for Mr. Brown that he is the originator of such a proposal? Mica insulation is largely used in the States; and, further than this, I have in my office the drawings of the first 5,000-H.P. alternator which I designed, long before Mr. Brown got out his design, in which the winding above referred to was adopted. In my latest design, however, it happens that the armature is wound with cotton-covered copper wire, No. 0, B. & S. gauge. It is true that the fields are wound with copper strip, but the insulation is tracing cloth. I submit that there is not the slightest foundation for the charge which has been made.

I think we owe Mr. Kapp a great debt of gratitude for having devoted some time to calculating the output, and so forth, of this machine. I fancy, though, that the time must have been hardly sufficient for him to work it out very fully—I mean, there are some points that would be a little difficult to work out in the short time that he had at his disposal—and that, probably, accounts for the fact that I hardly agree with him as to the proportions of copper which are required for the 33 periods and the 16 periods. I may say that I believe this machine is the first one which has been designed upon a certain principle—which always should rule the design of a large machine of new type of construction, where you have not got past experience to go upon—and that is, that you should reach the most economical efficiency. In this design the difficulty is to know how much copper and how much iron is to be put into the machine. If I had put more iron into the centre of the machine, I would have got a higher efficiency, and so also by putting in more copper. By that means we would have increased the efficiency and decreased the losses. In this design I have added material till I

reached a point at which the addition of 60 dollars' worth of material did not save 1 H.P. The 60 dollars was selected as the value of our H.P., for reasons which I need not mention here. Of course I could not expect Mr. Kapp, with the limited time at his disposal, to go into these minutiæ, but I am sure we owe him our thanks for taking as much trouble as he has in this matter. Mr. Kapp calculates the drop in volts, at full load, of my machine at 10 per cent. I make it rather less, but the difference is of no consequence whatever. Professor Forbes.

Professor Silvanus Thompson made some remarks on the elementary principles that govern parallel running, but did not give the slightest clue as to how we were to tell whether a certain machine would run in parallel or not, or what the waste current would be. I find that is really the one point on which I seem to differ from a great many others. I do find, from my experience of parallel running with a large number of types of machines and a certain number of different frequencies, that lowering the frequency improves, not only the parallel working, but the efficiency of parallel working.

Mr. Swinburne's remarks do not require very much reply. When he stated that flour mills do not use much power, and that, when water power is developed, the manufacturers themselves do not profit, he is strangely in error. It is not necessary to do more than point out the phenomenally rapid growth of St. Paul and Minneapolis, the sole reason for whose existence is the great water power (52,000 H.P.) which is being utilised almost entirely for flour-milling.

With regard to Mr. Ferranti's remarks, I must say that I absolutely deny, *in toto*, that any person has a right to say for one moment that the design of the machine that I have described to you has a plausible resemblance to the design of any machine that was put before us. I was not seeking to differentiate it; I was only seeking to get the best design I possibly could, and in doing so it happens that I have differentiated our design from those that were sent in infinitely more than they were differentiated among themselves.

Mr. Ferranti says I am wrong in saying that 20,000 volts,

Professor Forbes.

completely insulated, is so very good. He thinks that 10,000 volts, with one pole to ground, is better. I do not intend that either of my conductors shall ever be accidentally grounded, nor that any of the disastrous failures of Deptford shall be experienced by my company.

I think we owe a great debt of gratitude to Mr. Evershed for having given us some definite calculations with regard to the electro-motive force due to the inductive action of the conductors, and for showing us how valuable low frequency is for that reason. His results do not agree with those of Mr. Kennelly, or my own, but the point he makes is still important.

Mr. Crompton complains that I have not given the cost of producing a horse-power; and Mr. Sydney Walker, that I have not told the cost of the subway. I am very sorry they complain of it, when they can hardly have expected that I was likely to give them the estimated cost. I feel perfectly sure that, even when we have got the whole thing running, and we know the actual cost to a penny, we shall not announce it. There are some things which I have not been able to tell you, but I do not think that the Institution has any right to complain of my not having been candid enough to you, and not having given you as much information as I felt it was in my power to do.

The first rather surprising result of this discussion upon my mind has been to know that I am a discoverer, instead of a plain, straightforward plodder, and that low-frequency currents are a perfectly novel discovery. I must confess that has been a startling matter to me. In the next place, I have heard vast differences of opinion expressed by different members of the Institution, and I know the phases of mind through which they are passing. Not a single objection has been put forward that I had not weighed thoroughly, and I feel a great deal fortified in all the conclusions I have arrived at by the general result of the criticisms, however opposed to my views some of them might have appeared to be. You may think it is premature for me to say this at present, and perhaps it is; but although I thank most of those who have entered on this discussion, and who have given an expression of their

opinions, intended to benefit the progress of this work, and appreciate the motive and the value of what has been said by a great many, I can assure everyone here that I have not one particle less confidence in the perfect success of the work which is being carried out now than I had when I read my paper. In fact, from my own judgment of what has been said, I should say that my opinions have been strongly confirmed by the general result of the discussion which has been carried on on this paper. Professor Forbes.

The PRESIDENT: I think, Mr. Mordey, if you will forward to the Secretary any remarks in writing which you would like to see in the *Journal*, they will be referred to the Editing Committee and duly considered. The President.

Although the hour is very late, I feel sure that the meeting would not desire to close this evening's discussion without conveying to Professor Forbes our deep thanks for having brought this subject before us and having elicited such an important discussion. It was my very good fortune, with three other members of this Institution whom I see here to-night, to be conducted by Professor Forbes through these works that are being projected and carried out at Niagara. I am sure all of us appreciated, not only the works themselves, but the tremendous enthusiasm with which Professor Forbes has thrown himself into this magnificent work. We have had evidence before us to-night of his enthusiasm and of his nervousness: his suppressed anger every now and then doubtless hindered him from defending his position with that fluency with which we perhaps are more familiar. But if any one of us had laboured for years at a great work like this and had been as severely handled as Professor Forbes has been at different times, I do not think we should have been even as cool or collected as he was. I feel, as President of the Institution, a great pride that the conduct and superintendence of this great work has been left in the hands of one of our own members, and one who is a Member of our Council. The one lesson that I learned in my recent visit to America is this—that there is in one sense absolutely no difference of opinion between England and America: there is no feeling of national rivalry. An Englishman, if he knows anything, is received there as though he were an

The President.

American: and when an American comes over here, we receive him in exactly the same spirit. Thus we have an Englishman conducting on the other side of the Atlantic an immense work without a particle of jealousy being found throughout the whole of the States.

Now I am sure that one result of this great work in America will be to convince our friend Mr. Crompton that there is nothing like an alternating current after all. Somebody remarked that there was a kind of apologetic tone throughout the whole of Professor Forbes's paper in his defence of the alternating-current system against the direct current. Well, there was, and there is one great power he has had to contend against. He has told us to-night that he has converted the whole of the commission to his principles of alternating currents; but there is one behind that is not converted yet. There are only two men, I believe, who really want conversion—one is Mr. Crompton, and the other is Lord Kelvin—and I think the results of this work at Niagara will probably be to convert both of them. I shall take great pride in taking Mr. Crompton over there and bringing him back another man. I have taken great pains to read Professor Forbes's paper, and the great novelty in it is the advocacy of low frequency against high frequency. In his paper he has given us 18 distinct reasons in favour of low frequency; he has only been attacked on three or four, so that there remain 13 or 14 reasons that have scarcely been touched. I am on the side of Professor Forbes in being the strongest possible advocate of low frequency, and if time now permitted I would take up one subject, namely, the relation that exists between capacity and frequency. It has scarcely had even reference made to it. Mr. Mordey referred to it, as also did Professor Forbes in his paper; but I do ask everybody here to look more than they have done to the question of capacity as affecting the drop of potential, and with special reference to variation in frequency and distance apart of conductors.

We have had great differences of opinion expressed here, but there is no difference of opinion amongst us on this point—that we are deeply indebted to Professor Forbes for bringing this matter

before us; and I hope he will not be discouraged by the antagonistic criticism which his paper has met with. The President.

Professor Forbes: Not in the slightest.

The President: I hope he will go back to the other side of the Atlantic, and return to us when the work is finished and give us another paper describing what has been done, and tell us in his own familiar way of the success of the enterprise.

The vote was carried by acclamation.

The President: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Associate:

David Lowdon.

Student:

Henry Edward M. Kensit.

The meeting then adjourned.

ABSTRACTS.

H. EBERT and E. WIEDEMANN—EXPERIMENTS ON ELECTRO-DYNAMIC SCREENING AND ELECTRIC SHADOWS.

(*Wiedemann's Annalen, Vol.* 49, *No.* 5, *p.* 1.)

The authors have set themselves to thoroughly investigate the question already examined by Hertz and Stefan, and used for the purpose the parallel wires described by Lecher, connected by a conductor at one end, and at the other joined up to two condenser plates standing parallel to one another. When an exhausted and electrodeless tube is brought near the condenser, it is illuminated so long as the primary spark is passing, and consequently its extinction is a mark of screening from electrical oscillations.

In the experiments described in the paper, the vacuum tube and the screening body were placed with their length parallel to the axis of the condenser, and away from the parallel wires; and the order in which they were arranged was either—condenser, screen, tube,—or, condenser, tube, screen,—according as the experiments were on screening in front or behind. If the screen was not a wire or tube, but a metal plate, then the question of direction of its plane was also a factor to be noted.

It was found that screens of metal, or of any substance which screens behind itself, shield also in front and alongside themselves. The thinnest sheets of metal, even when transparent (like gold leaf), are screens; tubes also filled with a solution of an electrolyte, even when very weak; but insulators, such as glass and mica, do not screen. The dielectric in which the screening body is placed has not observable influence on the result. If the screen is to work at a distance of 1 cm. from the tube, it must be at least one-third to one-half of the length of the column of light, and when moved further away must be longer still to entirely quench the light. Tubes filled with rarefied gases do not screen in themselves, but become shields if made to glow by electric oscillation.

The authors sum up as follows:—Round conductors there is produced under the influence of electrical oscillation a *screened space*, both in front, behind, and alongside; and excitable bodies placed in this space are not excited so long as their capacity for excitation is not too great in comparison with the screening action at that point; and *luminous* gases take part in this property of conductors.

M. WIEN—A NEW FORM OF INDUCTION BALANCE.

(*Wiedemann's Annalen, No.* 6, 1893, *p.* 306.)

In this form of induction balance the arrangement is very simple, being that of a Wheatstone's bridge, two arms of which are coils having self-induction, and the other wo inductionless resistances. A sine current of $\frac{n}{2\pi}$ periods per second

flows through these arms, and the telephone which forms the bridge arm is silent when both resistances and self-inductions are proportional. Now, if a piece of metal be brought near one of the coils, the balance is disturbed. The coil itself is disturbed, and not the mutual induction of two coils, as in the old form of balance. When balance is restored by suitable adjustment of a secondary coil inside the other inductive coil, the time constant of the metal piece is equal to that of the compensating secondary coil. From this datum the conductance of various metals of similar form may be compared. The paper is an interesting one, and contains an archipelago of experiments surrounded by a sea of mathematics, in which the navigator has some difficulty in keeping his bark afloat.

F. KOHLRAUSCH—ON THE DETERMINATION OF ELECTRICAL RESISTANCE BY MEANS OF ALTERNATING CURRENTS.

(Wiedemann's Annalen, Vol. 49, No. 6, p. 225.)

This article is an amplification of the author's former work on the subject, in which he communicates the results of his experiments on the conditions of accuracy and trustworthiness of the method which he has introduced. He is of opinion that the difficulties, uncertainties, and the small differences which sometimes occur in the determination of the resistance of electrolytes by means of alternating currents, are due more to chemical than to electrical causes, and to the influence of temperature. The author's apparatus consisted of a Hartmann & Braun induction coil, with a platinum and quicksilver break under distilled water, working at about 85 interruptions per minute, and having 5 and 30 ohms resistance in the two coils; and the rest was as described in the former article. The results were as follows:—The accuracy of the placing of the bridge contact was one in ten thousand under favourable circumstances; and if the position of minimum sound is indefinite, owing to polarisation, self-induction, or capacity, the determination of the mid-point between two positions of similar amount of sound gives the resistance nearly correctly. Chaperon has introduced for telephone work a method of winding resistance coils up to 30,000 ohms; but for ordinary resistance coils it was found that up to 1,000 ohms they might be used with impunity, and up to 4,000 ohms the errors were ⅓ per cent. and more. Wien states that the error varies as the square of the resistance and the capacity, and the coils for large resistances should be subdivided, rather than wound all on the one bobbin, as the capacity is thereby diminished; and the use of a condenser in shunt to large resistances is recommended. The preparation of sufficiently inductionless coils is not difficult.

The error due to polarisation diminishes with the size of the electrode and the resistance: platinum electrodes of 10 and 35 square centimetres surface respectively in cells having resistances of 8 or 25 ohms, gave error of 3 or 1 per cent., and 1 and ⅓ per cent. differences in telephone and induction coil seemed to have no observable effect. If the product of surface and resistance be 250 ohm-square centimetres or more, the error which may be anticipated will not exceed 1 per cent. In using platinised electrodes a good coat of dull platinum must be used. Sine currents are much more influenced by polarisation.

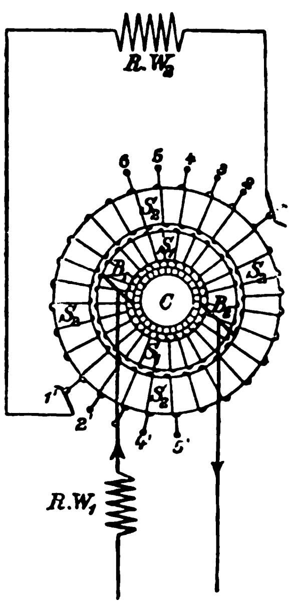
When large resistances are to be measured, special precautions must be taken; as, for instance, inductive effects of coil on bridge and telephone, which must be avoided by suitable means. In the case of water and very dilute solutions the electrostatic capacity of the liquid cell, which is proportional to the specific inductive capacity and the specific resistance of the liquid, disturbs the minimum; but this effect is not serious, and should not introduce 1 per cent. error.

When the resistances are of the order a hundred thousand ohms, the disturbances in the telephone due to static charges become annoying; but they can often be removed or mitigated by a suitable earth connection; but with resistances of the order megohms, the errors may amount to several per cent. Other small difficulties are due, in the water bath, to charges on the sides, and these increase as the size increases; they did not occur in a petroleum bath. A want of symmetry in the telephone sometimes occurs with large resistances, and is probably due to static charges. In such a case the telephone must be reversed and held in similar position, and the mean contact point used. This effect seemed to be greater with an induction coil having laminated core than with one having a metal tube over its core.

Dr. BEHN-ESCHENBURG—AN ALTERNATE-CURRENT MOTOR CAPABLE OF REGULATION.

(*Elektrotechnische Zeitschrift, No.* 21, 1893, *p.* 300.)

To avoid the difficulties which arose with the original Œrlikon motor due to the excessive current taken on light load, especially at starting, and other causes, the author has attempted to produce a motor whose speed and torque shall be as easily capable of regulation as the direct-current motor, and which shall be especially useful for tramways. The new motor consists essentially of the same parts as an ordinary direct-current motor—that is to say, of a set of inducing and a set of induced windings. In the place of the ordinary field winding of a direct-current motor, the exciting system can be with advantage arranged, as in the present Œrlikon motor, as a continuous iron ring without sharply defined poles, and wound in holes or slots as an open or closed Gramme or drum armature. The accompanying figure clearly shows the arrangement. The current comes from the generator along the leads to the brushes, B_1, B_2, and passes into the "field-magnet" winding, S_1, a closed Gramme ring The regulation of the current is performed by use of the resistance R W_1. The exciting system here is arranged similarly to the armature of a direct-current motor, and forms, with the collector, C, the rotating portion of the motor. The induced system is also



a Gramme ring similarly arranged. Now, as the motor as arranged has two poles, no currents can be formed in the circuit, S_2, except through a special connection made between suitable positions on the winding, so chosen that in the circuit so formed the sum of the electro-motive forces is not 0, and according to the choice of these points will the current vary; or the variation may be produced by a resistance, $R\ W_2$. Now with this arrangement the brushes B_1, B_2, obviously are always on the neutral point, and in practice it is found that there is not more sparking than in a direct-current motor. The arrangement of the field outside produces a motor of a less sparkless character, but the author has produced almost sparkless motors with this arrangement by subdividing the brushes and connecting corresponding pairs. The motor regulates perfectly. The author has made one of 6 H.P., which started with scarcely more current than would pass if the induced current were 0, and the speed could be altered from nothing to twice the so-called synchronous speed, and either field or armature could be outside. The efficiency is much higher than in the first Œrlikon motor.

M. JÜLLIG—A PHENOMENON OF ELECTRO-MAGNETIC ROTATION.

(*Elektrotechnische Zeitschrift*, *No.* 24, 1893, *p*, 345.)

If a hollow copper ball be suspended by a thread in the field of an electromagnet the coils of which are traversed by an alternating current, a turning moment is generally produced in the ball, causing it to rotate. This rotation, however, does not take place (1) if the centre of the ball lies in the plane at right angles to the plane of the magnet and to the line joining its poles; (2) if the centre of the ball lies in the plane of the magnet. In every other position the ball rotates without being artificially started; but it is necessary to work with large forces, as the rotation is due to a small difference of impulses. A diagram of the poles and ball in plan will show that each pole makes an induced current in the sphere, and the resultant current is more strongly acted on by one pole than by the other. The phenomenon might possibly be used in the construction of an ampere-hour meter for use with alternating currents.

P. CULMANN—ON MAGNETISATION BY VERY SMALL FORCES.

(*Elektrotechnische Zeitschrift*, *No.* 24, 1893, *p*. 345.)

In 1887 Lord Rayleigh published experiments which went to show that for forces from $4/10^5$ to $4/10^2$ C.G.S. units (unannealed Swedish iron wire) the magnetisation was directly proportional to the magnetising force. Herr Roessler in a recent series of articles has cast doubts on the accuracy of this conclusion, since he found deviations from proportionality amounting to 20 per cent., and he puts down Lord Rayleigh's failure to observe this to his method of observation, in which he only measured a presumably minute difference between quantities in themselves very small. The author does not agree with this objection, holding that Lord Rayleigh's method, in which differences alone are observed, must necessarily be more accurate than one in which the difference is between two

larger deflections, of which it forms but a small proportion; and though the observed quantity is small, much more sensitive apparatus can be used (in Lord Rayleigh's experiments an $H = 0.00004$ C.G.S. unit gives a deflection of 1·5 mm, and this is about 50 times the sensitiveness of Herr Roessler's method). The author has calculated the amount in actual length of the deviation from proportionality observed, which is 0·25 mm., and therefore concludes that we need not modify our views on the laws of very weak magnetisation as formed some time since on the basis of Lord Rayleigh's research.

W. H. HARVEY and F. BIRD—SOME NOTES ON BRUSH DISCHARGES IN GASES.

(*Philosophical Magazine, Vol.* 36, *No.* 218, *p.* 45.)

A remarkable difference between the behaviour of positive and negative electricity in high-frequency discharges, which was observed by the authors, forms the subject of this note. The arrangement of apparatus is as follows:—A small magneto excites the primary of an induction coil whose secondary can give about an inch spark with 8 volts on the primary. The terminals of the secondary are connected to two condensers of similar capacity, about $38/10^4$ microfarad, and also to the two balls of a spark gap. The other coatings of the condensers are connected through the primary of an air transformer, whose secondary is utilised to produce high potential and high-frequency brush discharges. The discharges, which were always vivid, were most brilliant when allowed to take place between a point and a plate having some capacity; and this plate had always a positive charge, though the discharge was of course oscillatory. The details of the experiment were varied in every way, but the plate always remained positive. If either end of the transformer secondary were connected to the plate, and the other end immersed in oil, the result was always the same—a strong positive charge. If the discharge was made to take place in dry hydrogen, the reverse result was obtained, the charge being negative. It appeared probable that a negative charge passes more easily in hydrogen than a positive. Oxygen behaved like air, the charge on the plate being positive.

These results have been to some extent anticipated, Guthrie having shown that a red-hot conductor in air retains a negative, but not a positive, charge.

F. UPPENBORN—THE ELECTRIC CENTRAL STATIONS OF MESSRS. SCHUCKERT & CO.: No. 4—ALTONA.

(*Elektrotechnische Zeitschrift, No.* 27, 1893, *p.* 377.)

The system used at Altona is similar to those of Barmen and Hanover—already described in this journal—in the use of comparatively large accumulator storage in combination with a three-wire system; but it differs in many interesting details, especially in the use of an earthed middle wire—a principle which found application here for the first time. The first difficulty was that of water, of which the supply was not sufficiently large, and a well was sunk to a depth of 200 feet to obtain the

amount required. The construction was begun on July 14th, 1891, the first run was on February 1st, 1892, and the official opening took place on March 15th; the preliminary installation being for 24,000 30-watt lamps, and the space provided sufficing for 80,000 lights.

The machinery consists at present of two boilers, by Berninghaus, of Duisburg, and two vertical triple-expansion engines with jet-condensers, by Schichau, of Elbing, fitted with all conveniences, and coupled directly to the dynamos, which are the ordinary Schuckert disc armature machines of 250 kilowatts output, of a height of about 10 feet, and having commutators about 6 feet in diameter, with 700 sections. To prevent side pull, the armature is adjustable between the poles; the volts are from 220 to 350, and the amperes 800 to 1,000. Complete details of connections and measuring apparatus are also given in the text, also of the accumulators and cable system. In order to avoid telephone and telegraph disturbances, the middle wire has been laid bare; and this has hitherto been quite successful in preventing disturbance, even when short-circuits take place. The cables are laid directly in the ground, as usual in Germany, and protected by U-iron at crossings, &c.

The tests resulted as follows:—7·4 lbs. of steam per 1 lb. of coal, 86·5 per cent. of the heat in the coal being utilised. The steam engines used 15·5 lbs. of steam per I.H.P., and the average efficiency of each steam dynamo was 82 per cent.; and 0·78 E.H.P. hour was produced per kilogramme of coal burnt. The automatic constant current system of charging the cells has been employed here with excellent results.

F. UPPENBORN—THE KÖNIGSBERG ELECTRICITY WORKS.

(*Elektrotechnische Zeitschrift*, *No.* 29, 1893, *p.* 413.)

This station is one which is of considerable interest among German stations, and is remarkable chiefly for two features: its circuits are on the five-wire system, and the mains are laid in conduits. The engines are of triple-expansion vertical type, and by Schichau, of Elbing; there are two of 200 H.P., and two of 100 H.P.; they run at 200 revolutions a minute, and are directly coupled each to two multipolar dynamos, which have the outside wires of their disc armatures arranged as commutators. Very full descriptions are given of the circuits of the five-wire supply, and also of the conduit in which the five conductors of bare copper strip are carried on insulators of the telegraph type. These mains are calculated for 60,000 lamps, but the present station can only supply 16,000. All details given are very full, and the financial results, in spite of the disproportionately heavy outlay on mains, are stated to be satisfactory.

J. BLONDIN—DETERMINATION BY ELECTRICAL METHODS OF THE MECHANICAL EQUIVALENT OF HEAT.

(*La Lumière Electrique*, *Vol.* 49, *No.* 31, *p.* 201.)

The methods employed for determining the mechanical equivalent of heat may be divided into two distinct groups. The first one requires only the measurement

of mechanical work and of heat, electrical phenomena appearing only between the transformation of mechanical energy into heat. In the second group it is necessary to measure electrical quantities, such as electro-motive force, current, and resistance, in addition to a heat quantity, and these are truly speaking, electrical methods; but until the last few years these were not considered sufficiently accurate for giving exact values of J. For this reason: Joule, Rowland, and Miculescu employed frictional methods. M. d'Arsonval showed, however, in 1891 that direct electrical methods were capable of yielding very reliable values of J, and recently Mr. Griffiths communicated to the Royal Society very accurate results obtained by a method of the second group.

Direct Experiments.

In 1843 Joule measured the amount of heat generated by the flow of an electric current in a conductor, for determining J. An iron core wound with a copper coil, was rotated rapidly between the poles of a powerful electro-magnet by means of a falling weight, from which the mechanical energy expended could be measured. Eight experiments yielded values varying from 322 to 572 kilogram-metres, the mean value being 460.

M. Leroux continued these experiments in 1857. He sent the current obtained from a dynamo through a platinum spiral in a calorimeter. The power was measured in the first experiment by means of a falling weight, and in a second experiment by the aid of a dynamometer. The former method gave from 442 to 469·87 kilogram-metres, and the latter 462·23, the mean being 458. In the following year Matteucci published a method slightly different to the above. An electromotor with a double winding was employed. When the secondary winding was doing no work, the motor was capable of lifting a weight P; but when the secondary circuit was closed on a platinum spiral resistance, the speed decreased, and to restore it to its previous value it was necessary to reduce the weight to p, the decrease of work corresponding to lifting the weights to a height h being $(P-p)\,h$, being converted into heat in the secondary circuit. One experiment gave 438·96 kilogram-metres, but others were not sufficiently consistent to allow of this being called an accurate method.

In M. Violle's experiments, made in 1870, a metallic disc was rotated between the poles of a powerful electro-magnet; the heat generated in the disc was measured in a calorimeter, and mechanical power produced by means of a falling weight. The mean of a series of experiments on a copper disc gave 435·2 kilogram-metres, and with an aluminium disc 434·9 kilogram-metres.

In 1891 M. d'Arsonval repeated this method by employing a cylinder of copper fixed in a calorimeter in the centre of which a magnet was rotated. Work was measured by the torque exerted on the cylinder. Values obtained varied between 421 and 427 kilogram-metres.

True Electrical Methods.

There are two direct electrical methods employed. In the first the electrical work of a cell and the heat generated are measured. The second depends on Joule's law with relation to the heat generated in a resisting circuit. The first method has been little employed on account of the number of secondary

phenomena produced in a cell. The Daniell cell is the only one which was studied by Bosscha and Joule. The former experimenter obtained in 1875 the figure 432·1 kilogram-metres, and both in 1859 obtained 419·5 kilogram-metres.

The method depending on Joule's law is very accurate, and has been frequently made use of. The amount of heat generated in unit of time in a circuit of resistance R is,

$$Q = \frac{1}{J} R I^2 = \frac{1}{J} E I = \frac{1}{J} \frac{E^2}{R}.$$

The determination of J, then, requires the measurement of heat, and of the three quantities E, I, or R. The accuracy of the results depends on the true determination of the ohm. The following table gives results obtained by various experiments:—

Year.		Authors.		Results.
1857	...	Quintus Julius (*Pogg. Ann.*, t. CL, p. 691)	...	399·7
1857	...	Weber (*Phil. Mag.*, 4th series, t. xxx.)...	...	432·1
1859	...	Lenz Weber...	...	396·4–478·2
1867	...	Joule	...	429·5
1878	...	Weber	...	428·15

In 1889 M. Dieterici employed a wire resistance immersed in petroleum in which the temperature was measured, the resistance forming one of the arms of a Wheatstone bridge. The current passing through the resistance was measured by means of a silver voltameter. The weight of ice melted in the calorimeter was a measure of the amount of heat generated. The value obtained was, $J = 4·2436 \times 10^7$ C. G. S, or 432·5 kilogram-metres.

The discrepancy between these figures and those of Mr. Rowland are accounted for by the fact that the specific heat of water at 0° C. is different to that at 15° C. Another source of error is in measuring the temperature of the wire, for, as pointed out by Mr. Griffiths, the temperature of the wire is slightly above that of the liquid surrounding it.

Mr. Griffiths's experiments, commenced in 1887, and finished at the end of 1892, are remarkable for their precision. After having tried in vain to obtain an alloy whose resistance did not alter with temperature, Mr. Griffiths determined to take a coil of platinum wire of which the temperature coefficient had been carefully determined. This coil was placed in a calorimeter having a temperature θ at any moment. But, since this coil is always slightly higher in temperature than the surrounding water, the temperature of the coil at the same instant will be $\theta_1 + B$, B being an unknown quantity, determined experimentally. Then, if R is the true resistance of the coil at a temperature θ, its resistance at the moment when the calorimeter is at a temperature θ_1 is,

$$R^1 = R\left[1 + k(\theta_1 + \quad - \theta)\right],$$

k being the coefficient of variation of resistance of platinum.

The measurement of a certain quantity of heat during a time dt, $\frac{IE^2}{R}\,dt$ necessitated the use of an electrical clock with a seconds pendulum.

After making the necessary corrections for radiation, &c., the following results were arrived at:—

Groups.	15°.	20°.	25°.	Mean.
A	$4{\cdot}1940 \times 10^7$	$4{\cdot}1940 \times 10^7$	$4{\cdot}1940 \times 10^7$	$4{\cdot}1940 \times 10^7$
B	$4{\cdot}1930 \times 10^7$	$4{\cdot}1941 \times 10^7$	$4{\cdot}1949 \times 10^7$	$4{\cdot}1940 \times 10^7$
C	$4{\cdot}1939 \times 10^7$	$4{\cdot}1938 \times 10^7$	$4{\cdot}1937 \times 10^7$	$4{\cdot}1938 \times 10^7$
D	$4{\cdot}1940 \times 10^7$	$4{\cdot}1939 \times 10^7$	$4{\cdot}1940 \times 10^7$	$4{\cdot}1940 \times 10^7$
E	$4{\cdot}1938 \times 10^7$	$4{\cdot}1940 \times 10^7$	$4{\cdot}1943 \times 10^7$	$4{\cdot}1940 \times 10^7$

The experiments were made with equal weights of water.

Then, (1) if as a unit of resistance the B.A. unit of 1892 is taken, (2) and for the value of the standard Clark cell at the Cavendish Laboratory we take 1·4342 volts at 15° C., (3) and as the thermal unit the amount of heat required to raise the mass of 1 gramme of water at 15° by 1° C. on the hydrogen scale, the most probable value of the mechanical equivalent of heat will be

$$J = 4{\cdot}1940 \times 10^7 \text{ C.G.S.}$$

This value corresponds to 427·45 kilogram-metres for Greenwich latitude, where = 981·17. It differs by less than $\frac{1}{830}$ from Mr. Rowland's results, and by about $\frac{1}{500}$ from those of M. Miculescu.

A mean of Joule's figures, making allowance for probable inaccuracies, would agree within $\frac{1}{350}$. A mean of all results would agree within $\frac{1}{4250}$.

The above comparison shows that the approximation to the value of J as found by Mr. Griffiths agrees within at least $\frac{1}{500}$ of its true value, and perhaps within $\frac{1}{1000}$.

C. JACQUIN—DISTRIBUTION OF ELECTRICAL ENERGY BY POLYPHASE AND CONTINUOUS CURRENTS AT BOCKENHEIM.

(*La Lumière Electrique, Vol.* 49, *No.* 31, *p.* 208.)

The author first points out that an electricity supply station should be capable of supplying current for power as well as for lighting purposes, thus obtaining a more even load-factor. Factories which have previously used steam power will not readily adopt electrical power, perhaps through force of habit, or on account of the difficulties which present themselves for favourably disposing of the old plant; and in many cases will not use electro-motors, in view of the apparent cost of installation, unless great economy of working or other advantages present themselves.

Electrical energy at the present moment is charged so much for by supply companies that even for small powers it pays to instal gas or petroleum engines in the place of electro-motors, and with larger outputs the difference in cost is still more appreciable. It is therefore essential that the price at which electrical energy is supplied should be sufficiently low to induce customers to adopt the use of motors, but it would only be for outputs up to about 10 H.P., since for anything above this it pays to use steam or gas as a motive power. The station

should therefore not be in a large industrial city, but rather in a town occupied by small works, in which only a small amount of power is required. This is one of the reasons why electrical power has been so generally adopted in America, where new towns are continually being formed. But in France and on the Continent it is rarer to find places offering the necessary conditions.

At Bockenheim, a small town on the northern outskirts of Frankfort-on-Main, factories have developed during the last few years, and form part of a small industry, consisting mostly of saw-mills, joiners' shops, screw and electrical factories. As ground is not expensive at Bockenheim, the first cost of a station would be somewhat reduced. These circumstances induced M. Lahmeyer to carry out on a large scale at Bockenheim the system he had so successfully shown at the Frankfort Exhibition, and which may be briefly described as follows:—

Current is transmitted from the station to the centre of the town at a sufficiently high potential to reduce the cost of the mains, and yet not too high to be detrimental to the good working of motors. With, say, 600 or 1,000 volts, the motors might be connected directly to the high-tension mains, which would run through the principal thoroughfares, and for the lighting of which arc lamps would be run in series from these mains. Next to these would be laid low-tension mains, coupled to the former through special transformers, reducing the pressure from 1,000, or 600 volts to 100. The low-tension mains are connected from a point near the generating station to low tension dynamos, which are also employed for exciting the high-tension machines. Incandescent lamps, isolated arc lamps, and small motors are run in parallel from the low-tension mains.

Since the introduction of polyphase currents and their brilliant success at Lauffen, it was clear to M. Lahmeyer that his system might be rendered more simple and practical by their use. The commutators on direct-current dynamos, working at 1,000 volts, would offer considerable trouble when compared with the simple collectors on polyphase machines. But matters were simplified still further by employing a fixed armature for the latter machines, producing triphase currents at only 80 volts in the station, the pressure being raised to 600 volts on the feeders by similar transformers to those used at Lauffen; these feeders being connected to three-wire ring mains. A sub-station contains a special transformer the primary of which is connected to the three wires of the high-tension mains, the secondary delivering a continuous current at 110 volts into two-wire low-tension mains, to which lamps, motors, or accumulators are connected. The dynamos used for exciting the triphase alternators also feed into the continuous-current mains at a point near the station, and are capable during periods of light load of alone supplying current.

The station is at some distance from Bockenheim. The ground space occupies 3,600 square metres, thus allowing for future extensions. The ground is 72 metres long by 50 metres wide, in the centre of which a building 25 × 50 × 9 metres has been erected, consisting of a machine hall, boiler house, and offices. The boiler house measures 7·5 × 17·5 × 12·5 metres, and contains two boilers, with room for a third. Supply water is obtained from the mains. Each boiler has a heating surface of 90 square metres, and can produce 350 kilogrammes of steam per hour at 10 atmospheres pressure, with a consumption

of 80 grammes of coal per kilogramme of steam. The machine hall has the same dimensions as the boiler house. Two independent groups of machines are placed in parallel lines. The switch-board is at the end of the hall, with a raised platform 3 metres from the floor. There are two horizontal compound Corliss engines, with tandem cylinders, of 250 H.P. each.

The field magnets of the triphase alternators are similar to those used on the Lauffen machines, each disc having 24 claws on its periphery, and the two discs when in position give 48 polar surfaces of alternating polarity. The diameter of these field magnets is 2·80 metres, which are worked directly from the engine shaft. The 50-H.P. continuous-current dynamos are belt-driven from the engine fly-wheels, and run at 600 revolutions per minute. In a chamber under the dynamo room is placed a condenser, working at 650 mm. vacuum. The hot water from the condenser runs in thin streams down a number of boards arranged in steps, and, after being cooled in this way, is used for cooling the condenser; by this simple method it is possible to cool 40,000 litres of water per hour. The triphase alternators are very similar to those used at Lauffen. The fixed armature consists of 144 copper bars 20 mm. in diameter, insulated with asbestos, and so connected that they form three distinct circuits, each of 48 coils, these circuits ending in three terminals on the dynamo. The machines are of the simplest construction. Each triphase machine has an output of 150 kilowatts, giving 1,850 amperes at 80 volts. The two four-pole continuous-current dynamos, of the Lahmeyer type, have an output of 36 kilowatts, 30 amperes at 120 volts.

J. GARNIER—THE EFFECT OF ELECTRICITY ON THE CARBURATION OF IRON.

(*Comptes Rendus*, *Vol.* 116, *No.* 25, *p.* 1449.)

The process of heating iron in carbon for hardening its surface is very old, and still somewhat obscure. Reamur was the first to attempt to clear the process of its mysteries, but did not satisfactorily do so. Case-hardening is carried out to-day as it was centuries ago, save that the apparatus employed has assumed larger dimensions; but it is still necessary to heat the iron for long periods, numerous empirical additions being made to the carbon—such as salt, leather, horn, &c.—which have but helped to add to the mysteries without shortening the process. The author thought that electricity might be of aid, carbon being used as an anode and iron as the cathode. Experiments were consequently carried out in M. Hillairet's workshops.

A carbon rod and a bar of steel, with only $\frac{1}{1000}$ of carbon, were put end to end in a fireproof tube, placed horizontally in a small reverberatory furnace. The necessary current was obtained from a Gramme dynamo, carbon forming the positive and the iron the negative pole.

It was not advisable to establish an arc, on account of the expense in actual practice, and also owing to certain chemical objections, although by so doing carbon would be effectually carried from positive to negative. A current of 55 amperes at 7 volts was maintained for three hours, at the end of which

time the steel bar was taken out and rapidly plunged in water. The end which had been opposite to the carbon effectually cut glass, and, on being ground down on an emery wheel, showed that carburation had taken place to a depth of about 10 mm. The end of the carbon rod which had made contact was well pitted.

The above operation was repeated, and was found to require a temperature of about 900° C., or 1,000° C. at the most.

In the next experiment, the carbon rod was replaced by a bar of steel similar to the one it was necessary to carburate, the two bars being separated by about 1 centimetre, which space was filled up with charcoal. A current of 55 amperes at 2·5 volts was maintained for three hours. Both bars were then withdrawn. The one forming the anode was not altered; its edges had remained sharp, and its surface not appreciably hardened. The bar forming the cathode, on the contrary, was deeply carburated, especially on the underneath side, which had melted in places, owing, no doubt, to the concentration of heat on that side. It would have been advisable to rotate the tube.

The author comes to the conclusion that the carburation of iron takes place with great rapidity at 1,000° with a current of 50 amperes at 2·5 volts.

ANON.—THE MANUFACTURE OF MICA PLATES.

(*La Lumière Electrique, Vol.* 49, *No.* 28, *p.* 77.)

Small pieces of mica, the largest of which might be about 40 sq. cm., are made to fall through a long tube into a small tank mounted on wheels; into this tank is pumped a cement consisting of gum lac dissolved in alcohol. After having mixed up a sufficient quantity of the mica and cement to make the necessary thickness of plate, a piece of sheet iron is placed on the surface; this operation is repeated until the tank is full of layers of cemented mica separated by sheet-iron plates. The whole is then subjected to gentle heat and compressed, excess of cement being allowed to flow away. The mica plates when finished can be lifted away from the tank. By the above method mica plates can be manufactured which are easy to work and of sufficient strength for ordinary purposes.

F. CHÉDEVILLE—THE UTILISATION OF SMALL WATERFALLS.

(*La Lumière Electrique, Vol.* 48, *No.* 22, *p.* 401.)

The author makes certain remarks on a paper read by M. Hillairet before La Société des Electriciens, on the utilisation of waterfalls for the transmission of power. Useful waterfalls may be divided into two classes—those which are close to some highway, and those which are far away. The former are of considerable value, and may not require the aid of electricity for purposes of transmission. On the other hand, those which are distant will require an electrical plant for their utilisation, but in this case the necessary capital must not represent a very large percentage of the total outlay.

Although the advantages of hydraulic over steam power are numerous, such as greater efficiency, diminished working expenses, and smaller capital outlay and

depreciation, it is at times difficult to obtain a waterfall with sufficient regularity of flow and other necessary conditions.

But in many cases falls which at first sight would appear useless might be utilised by sufficiently altering the configuration of the neighbouring ground. In the case where power is utilised for electric lighting, there are two methods of using the fall—either to let the power be wasted in the daytime and only run the machines during hours of lighting, or to charge accumulators during the daytime and discharge them in the evening.

The first method is not to be recommended, mainly on account of the waste of available energy and an uneven load-factor. The second method is, then, the more rational: the accumulators would act as potential regulators, and as a reserve in cases of breakdown. But if these accumulators are to take a part of the load, first costs will be considerably increased, and depreciation may become very rapid, especially in the case of accumulators of large capacity. Their low efficiency would also prohibit their use, especially in cases where available power is small. The use of high tension in this case would become impracticable. The great drawback to both the above methods is that no provision is made for compensating the great variations in supply which take place from time to time. The output of the station will depend on the minimum power available, which a certain periods may become very small. To ensure regularity of supply, it would then be necessary to instal a gas or oil engine, but would raise first costs and working expenses. The most natural solution to the above difficulties is in making the water itself fulfil the functions of an accumulator.

The ground in the vicinity of the fall would probably be of no great value, and might, in consequence, be utilised for building a reservoir. Variations of water supply would thus be got rid of and a greater output obtained. If, moreover, the ground occupied by the reservoir is sloping, a considerable increase in the height of fall may be obtained, especially if several partial falls have been collected into one. Assuming that each lamp is alight, on the average, for five hours per day—which is certainly too much—then by using the reservoir one would get 15 times more power from the fall than if it were used in the ordinary way only during the hours of lighting, and certainly three times more than with the use of accumulators without the reservoir. There would also be a great advantage in being able to obtain a considerable amount of power on special occasions, without the expense and depreciation of a battery. If high-tension alternating currents be used, and transmitted to neighbouring towns with sub stations, one is more at liberty to choose an advantageous spot for the reservoir. This system will necessitate the use of larger machines, on account of the increased power of the fall. The machines will be more efficient, and working expenses reduced, on account of the shorter hours of working. As an example of the advantages to be derived from the above system the author describes one of M. Claude's installations.

The necessary power is supplied from one of the tributaries of the river Marne, where, without much cost, a 10-metre fall can be obtained.

The village to which it is desired to transmit power is at a distance of 1·5 kilometres from the fall. The surplus power might be utilised by several small villages in the vicinity.

The flow was carefully measured several times during a year which included two exceptionally dry periods. The results of these observations are plotted in a curve. The limits of flow were found to vary from 1,300 to 35 litres per second. The average flow was 225 litres per second. During a period of frost it fell to 100 litres per second. If the fall were utilised in its natural condition, one could depend only on 80 litres per second in winter, and with no regularity. Assuming an output of 75 per cent. for the turbine, 80 per cent. for the dynamo, and 90 per cent. for the line, there would be 5·76 H.P. available at the terminals of the lamps, sufficient for 75 16-candle-power lamps.

Now, if a battery of accumulators were employed, and six hours be the maximum duration of lighting, the fall would be utilised four times longer, and the available power becomes 16 H.P. during six hours, or sufficient for 240 16-candle-power lamps. The stream under consideration flows in a confined valley about 100 to 200 metres wide; at one point there is a waterfall of about 5 metres in height, and 1 kilometre away another fall, with a slope of about 6 metres between them. The valley at the second fall is only about 50 metres wide, thus being eminently adapted for the construction of a dam. The reservoir would occupy an area of 15 hectares, and if we assume a depth of 12 metres, the contents would amount to 1,800,000 cubic metres.

Assuming a mean fall of 16 metres, and allowing 75 per cent. efficiency for turbines, 90 per cent. for dynamo, 90 per cent. for line and transformers, 156 H.P. would be available at the lamp terminals, and would therefore be capable of lighting more than 2,300 16-candle-power lamps during six hours in winter and two hours in summer.

It is thus possible to obtain 25 times more power than from the fall in its natural condition, and seven times more than with the use of accumulators.

The above case is then an example of how an apparently insignificant fall may be made to develop comparatively large amounts of power.

G. RICHARD—ALUMINIUM AND ITS ELECTRO-METALLURGY.

(*La Lumière Electrique, Vol.* 49, *No.* 28, *p.* 67.)

A suitable solder for aluminium is being sought for by a great many people.

Mr. R. Heaton proposes the use of an alloy of 45 parts of tin to 11 parts of aluminium, made without a flux, by simply mixing the two molten metals. The parts of the objects to be soldered are heated to a red heat, then a piece of the solder is picked up by a hot brass wire and spread over the hot aluminium. This process requires no flux.

Mr. J. W. Richards employs an alloy consisting of 1 part of aluminium, 8 parts of zinc, 32 parts of tin, and 1 part of tin containing 5 per cent. of phosphorus. The phosphorus acts as a deoxidising agent, preventing the formation of an oxide on the surface of the metal, which is so detrimental to the process of soldering.

MM. Weyner and Guhrs employ as flux a mixture of 80 parts of stearic acid-10 parts of chloride of zinc, and 10 parts of chloride of tin. This flux yields excellent results with a solder consisting of 80 parts of tin and 20 of zinc.

M. J. Novel communicated to L'Academie des Sciences the following results:—Aluminium may be soldered with the following alloy without detriment to the metal. The process is quick and the joint strong. A tinsmith's iron or the blowpipe may be used with success. It is cheaper than most solders, and has the advantage of not oxidising the metal. The following solders may be used for iron, tin, brass, nickel, &c., with great success:—

Solder No. 1.

Pure tin without alloy ... ... Melts at 250°.

Solder No. 2.

Pure tin	1,000 gr.	Melts at 280° or 300°.
Fine lead	50 gr.	

Solder No. 3.

Pure tin	1,000 gr.	Melts at 280° or 300°.
Pure zinc	50 gr.	

The three solders do not alter the colour of the metal. It is preferable to use a soldering iron of pure nickel.

Solder No. 4.

Pure tin	1,000 gr.	Melts at 350° or 450°.
Copper	10 to 15 gr.	

Solder No. 5.

Pure tin	1,000 gr.	Melts at 350° or 450°.
Pure nickel...	10 to 15 gr.	

These two solders impart a slightly yellow tint to the aluminium, but have the advantage of melting at a higher temperature, and are harder and stronger. Aluminium so soldered might replace to great advantages numerous metals which so readily oxidise.

Solder No. 6.

Pure tin	900 gr.	Melts between 350° and 450°.
Copper	100 gr.	
Bismuth	2 to 3 gr.	

This solder has a golden colour, and may be used for soldering aluminium bronze. Its colour may be altered by the addition of more or less copper.

In the process recently published by M. Willson for the electrolytic manufacture of aluminium bronze, a plumbago crucible is used, forming the anode of a circuit. Small pieces of copper are placed in this crucible, covered with a layer of finely divided aluminium, in the proportion of 2 of copper to 1 of aluminium. Current passes through an upper carbon, which at first touches the contents of the crucible and is then drawn away to form an arc. Powdered carbon is also placed in the crucible, which reduces the consumption of the upper carbon rod, and also prevents ebullition, which would cause considerable variations in the current. With a charge of 2 to 3 kilogrammes a current of 200 amperes and 50 volts is necessary. With pure aluminium it is only necessary to add about 15 per cent. of carbon. Tar may be mixed up with the aluminium. The light hydrocarbons are driven off by heating, and a dry carbon powder left after decomposition of the products. It is necessary to heat the crucible up to the melting temperature of copper.

Kreinsen devised an electric furnace for the manufacture of aluminium bronze. The crucible is coated with platinum, through which current passes, and heat is thus produced for the crucible. At the top of the crucible is fixed a carbon disc. Current passes from its periphery to a metal rod, which fuses, and the molten metal falls into the hot crucible containing the necessary substances for the manufacture of aluminium bronze.

M. Lébédeff recently suggested the following chemical method:—Silicate of aluminium and a necessary flux, such as fluoride of calcium, are placed at the bottom of a crucible; to this is added a sulphate of the metal to be alloyed with aluminium. The silicate will form the bottom layer, and the sulphate the top layer. An inverted plumbago crucible is so supported as just to dip into the sulphate. The mixture is constantly stirred, in order to renew the surfaces of contact of the two layers. Owing to the carbonic oxide produced by the presence of the plumbago crucible, reduction of the sulphate will take place, and the metal will alloy with the silicate of aluminium.

In the process ordinarily employed for obtaining aluminium from bauxite by the use of carbonate of soda to form aluminate of soda, from which aluminium is finally precipitated, the metal obtained has always an impurity of about 2 or 3 per cent. of silicon, which is found very injurious for purposes of manufacture.

In order to eliminate the silicon, MM. Hand and Kunheim propose to add to the crucible a mixture of bauxite, carbonate of soda, and phosphoric acid (in the form of phosphate of soda), and in the proportion of 1½ equivalent of phosphoric acid to 1 equivalent of silicon present in the bauxite. After filtration of the aluminate of soda, the aluminium is precipitated as in the ordinary way with carbonic acid.

— LAGARDE—NOTE ON THE ELECTRICAL RESISTANCE OF PURE COPPER WIRE.

(*Annales Télégraphiques, Vol.* 20, *No.* 3, *p.* 235.)

Owing to the difference in the specific resistance between high-conductivity copper wire now used, and the results of previous determinations, the author thought it advisable to carry out a series of experiments in order to find the true value.

Copper wire was obtained from the firm of Grammont, of Pont de Chérui. The length of wire was 100 metres, 0·001 metre diameter, and was made from electrolytically deposited copper. A chemical analysis of 10 grammes of the wire showed that it contained inappreciable traces of iron and bismuth.

To determine the electrical resistance of the wire, the author made use of the apparatus described in the *Annales Télégraphiques* (vol. xv., p. 409). The wire placed on the drum was 100·48 metres in length, which was measured directly, and also determined from its weight. The concordance of the two results showed that the cross section was very uniform, the wire having been drawn through a diamond die. The density of the metal deduced from the weight of 1 metre was 9·05. The resistance was first measured at 0°, and then at temperatures varying between 0° and 40°. The wire measured 1·968 legal ohms at 0°, or 19·58 ohms per kilometre at 0°.

In view of the great purity of the sample, the value 19·58 ohms for 1,000 metres of 1 mm. diameter wire at 0° may be considered as reliable. The value generally taken for this wire is 20·34 ohms, or 96·26 per cent. conductivity, as compared to the sample in question. The following table gives the coefficients of variation with temperature :—

Temperatures.	Resistances.	Coefficient of Variation between 0° and each Temperature.
0°	ω 1·968	—
16·75°	2·119	0·00458
18·5°	2·129	0·00442
22°	2·159	0·00441
27·2°	2·213	0·00457
31 5°	2·240	0·00438
32°	2·241	0·00433
40°	2·319	0·00445
41°	2·331	0·00449

the values in the last column being obtained from the formula, $R_t = R_0\ (1 + a\ t)$. The mean value of $a = 0·00445$. One of the important conclusions arrived at in the previous article (*Annales Télégraphiques*, vol. xv., p. 409) was that the coefficient of variation of resistance with temperature, in the case of copper or bronze wire, is proportional to the electrical conductivity of that wire. For instance, the coefficient in the case of a copper wire of 90 per cent. conductivity would be

$$\frac{a_1}{0\ 00445} = \frac{90}{100}.$$

$a_1 = 0·00400.$

Conductivity of Copper as Compared to Sample.	Coefficients.
% 90	0·00400
91	0·00405
92	0·00409
93	0·00414
94	0·00418
95	0·00423
96	0·00427
97	0·00432
98	0·00436
99	0·00441
100	0·00445

The copper which was previously considered as pure, but which now has 96·26 per cent. conductivity as compared to the present sample, would have the value $\alpha = 0{\cdot}00428$ for the coefficient. The previous experimental value was 0·00388. The difference is no doubt due to the fact that the tests were made at 100°, at which temperature the law of variation is not the same as between 0° and 40°, the limits of temperature of the present test. Copper of 98 per cent. conductivity would, with comparison to the sample in question, have only 94·33 per cent. conductivity, and the value of α would be 0·00420.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals from JUNE to NOVEMBER, 1893.

S. denotes a series of articles. I. denotes fully illustrated.

LIGHTING AND POWER.

J. Sahulka—The Application of Condensers to Alternate-Current Working.—*E. T. Z.*, 1893, No. 22, p. 317 (S. I.).

M. Büttner—General Remarks on Accumulator-driven Tramways.—*E. T. Z.*, 1893, No. 25, p. 354.

K. Wilkens—Hartmann & Braun's New Arc Lamp.—*E. T. Z.*, 1893, No. 26, p. 370 (I.).

F. Chédeville—The Utilisation of Small Waterfalls.—*Lum. El.*, vol. 48, No. 22, p. 401 (I.).

G. Richard—Electric Tram and Railways.—*Lum. El.*, vol. 48, No. 23, p. 458; vol. 49, No. 40, p. 22, No. 32, p. 255 (S. I.).

J. P. Anney—Distribution of Electric Energy.—*Lum. El.*, vol. 48, No. 23, p. 472, No. 25, p. 570 (S. I.).

G. Richard—Recent Arc Lamps.—*Lum. El.*, vol. 48, No. 25, p. 555, No. 33, p. 313; vol. 50, No. 43, p. 171 (S. I.).

F. Uppenborn—The Electric Central Stations of Messrs. Schuckert & Co.: IV.—The Altona Works.—*E. T. Z.*, No. 27, 1893, p. 377 (S. I.).

J. Sahulka—The Theory of the Thomson (Brown) Motor for Ordinary Alternate Currents.—*E. T. Z.*, No. 27, 1893, p. 391 (I.).

R. Ulbricht—The Electricity Works for the Dresden Railway Stations.—*E. T. Z.*, No. 28, 1893, p. 404 (I.).

F. Uppenborn—The Königsberg Electricity Works.—*E. T. Z.*, No. 29, 1893, p. 413 (I.).

C. Solter—The Pergine Electric Light and Power Station.—*E. T. Z.*, No. 30, 1893, p. 428 (I.).

G. Richard—The Mechanical Applications of Electricity.—*Lum. El.*, vol. 49, No. 27, p. 13, No. 29, p. 112, No. 31, p. 214, No. 36, p. 458, No. 41, p 67, No. 42, p. 115 (S. I.).

E. Zetsche—Electric Locking Apparatus for Railway Signals.—*Lum. El.*, vol. 49, No. 27, p. 22 (I.).

Anon.—Electricity applied to Traction of Railway Trains.—*Lum. El.*, vol. 49, No. 28, p 83.

C. JACQUIN—The Electric Light at Frankfort-on-Main.—*Lum. El.*, vol. 49, No. 29, p. 124.

ANON.—The Rice System of Alternate-Current Force-Transmission.—*Lum. El.*, vol. 49, No. 29. p. 132 (I.).

— DESROZIERS—On the Transmission and Distribution of Power.—*Bull. Soc. Int.*, vol. 10, No. 98, p. 231 (I.).

— HILLAIRET—Remarks on the latter Paper.—*Id.*, p. 264.

J. VIOLLE—Light and Heat of the Electric Arc.—*Bull. Soc. Int.*, vol. 10, No. 99, p. 291.

C. JACQUIN—The Bockenheim Polyphase and Direct-Current Transmission.—*Lum. El.*, vol. 49, No. 31, p. 208, No. 33, p. 308, No. 32, p. 269 (I.).

W. DE FONVIELLE—Electric Illuminations in Paris.—*Lum. El.*, vol. 49, No. 31, p. 224.

ANON.—The Lauterbrunnen-Mürren Railway.—*Lum. El.*, vol. 49, No. 33, p. 327.

G. RICHARD—Incandescent Lamps.—*Lum. El.*, vol. 49, No. 34, p. 356 (I.).

F. UPPENBORN—The Cassel Electricity Works.—*E. T. Z.*, No. 31, 1893, p. 437 (I.).

ANON.—The Chur Electricity Works.—*E. T. Z.*, No. 31, 1893, p. 444 (I.).

ANON.—New Shunt Arc Lamp by Körting & Mathiesen.—*E. T. Z.*, No. 33, 1893, p. 479 (I.).

F. JEHL—The most Economical Position for a Central Station.—*E. T. Z.*, No. 34, 1893, p. 486 (I.).

G. RICHARD—Electric Welding.—*Lum. El.*, vol. 49, No. 35, p. 410 (I.).

A. BLONDEL—The Electric Illumination of Lighthouses.—*Lum. El.*, vol. 49, No. 35, p. 440 (I.).

ANON.—Electric Train Lighting.—*Lum. El.*, vol. 49, No. 36, p. 475, No 37, p. 519, No. 38, p. 573 (S. I.).

ANON.—Dangers of Electric Conduits in Proximity to Gas Pipes.—*Lum. El.*, vol. 49, No. 36, p. 483 (I.).

A. BLONDEL—New Researches on the Alternate-Current Arc.—*Lum El.*, vol. 49, No. 37, p. 501, No. 38, p. 557, No. 39, p. 608 (I.).

ANON.—High-Tension Fuses.—*Id.*, p. 528 (I.).

A. HESS—The Projected Brussels Electric Underground Railway.—*Lum. El.*, vol. 49, No. 39, p. 601 (I.).

F. UPPENBORN—The Central Stations of Schuckert & Co.: V.—The Town of Christiania.—*E. T. Z.*, 1893, No. 36, p. 509 (I.).

ANON.—Œrlikon Electric Travelling Crane.—*Id.*, p. 528 (I.).

S. KALISCHER—Electric Currents between Gas and Water Pipes.—*E. T. Z.*, 1893, No. 38, p. 548.

ANON.—New Installation System of Hartmann & Braun.—*Id.*, p. 549 (I.).

G. MARRIOTTI—On Direct-Current Motors in Constant Potential Cable Networks.—*E. T. Z.*, 1893, No. 39, p. 557 (I.).

B. SZAPIRO—Regulating Pressure of the Alternate-Current Arc in its relation to the Form of the E.M.F. Curve.—*E. T. Z.*, 1893, No. 39, p. 558.

ANON.—The Erding Municipal Light Station by Siemens & Halske.—*Id.*, p. 558 (I.).

— LEFEVRE—The Electric Lighting of Langenfelde and Stellingen.—*Bull. Soc. Belge*, vol. 10, No. 5, p. 187.

P. MARCILLAC—The Tivoli-Rome Transmission of Electric Energy.—*Lum. El.*, vol 49, No. 40, p. 7, vol. 50, No. 43, p. 156 (I.).

H. DE GRAFFIGNY—The Electric Lighting of Envermeu.—*Id.*, p. 27.

ANON.—The Electric Lighting of Railway Trains.—*Id.*, p. 29, No. 41, p. 77.

ANON.—The Seger Steam Turbine.—*Id.*, p. 31 (I.).

C. JACQUIN—The Pollak Commutator for Charging Accumulators with Alternate Currents.—*Lum El.*, vol. 49, No. 41, p. 54 (I.).

F. UPPENBORN—Central Stations of Siemens & Halske : I.—Helsingborg.—*E. T. Z.*, 1833, No. 40, p. 569 (I.).

DUNCAN, ROWLAND, and TODD.—The Arc under Pressure.—*E. T. Z.*, 1893, No. 42, p. 602 (I.).

F. UPPENBORN—The Bockenheim Electricity Station.—*E. T. Z.*, 1893, No. 43, p. 609 (I.).

DYNAMO AND MOTOR DESIGN.

L. BAUMGARDT—A Ring Armature for Multipolar High-Tension Machines.—*E. T. Z.*, 1893, No. 23, p. 331 (I.).

K. PICHELMAYER—A Contribution to the Study of Alternators.—*E. T. Z.*, 1893, No. 24, p. 344 (I.).

E. SCHULZ—Experimental and Theoretical Researches on Dynamos.—*E. T. Z.*, 1893, No. 26, p. 367 (S. I.).

E. EGGER—Magnetic Reactions in the Construction and Working of Dynamos and Motors.—*E. T. Z.*, 1893, No. 26, p. 370.

G. RICHARD—Details of Construction of Dynamo Machines.—*Lum. El.*, vol. 48, No. 22, p. 405; vol. 49, No. 30, p. 162, No. 37, p. 508 (S. I.).

F. GUILBERT—The Alternate-Current Motors of MM. Hutin and Leblanc.—*Lum. El.*, vol. 48, No. 23, p. 451 (I.).

W. E. AYRTON—Dynamo Testing.—*Lum. El.*, vol 48, No. 23, p. 457.

A. BLONDEL—Testing of Alternators.—*Lum. El.*, vol. 48, No. 23, p. 467 (I.).

P. BOUCHEROT—Attempts to produce Direct-Current Dynamos without Collectors.—*Lum. El.*, vol. 49, No. 27, p. 26.

F. GUILBERT—An Inductionless Alternator by Hutin and Leblanc.—*Lum. El.*, vol. 49, No. 28, p. 51 (I.).

A. HESS—Steam Dynamos for the Navy.—*Lum. El.*, vol. 49, No. 28, p. 64 (I.).

F. GUILBERT—Improvements in Alternate-Current Motors.—*Lum. El.*, vol. 49, No. 30, p. 151 (I.).

A. HESS—Portable Apparatus for Electric Lighting.—*Lum. El.*, vol. 49, No. 30, p. 154 (I.).

W. RECHNIEWSKI—Sparking at the Brushes.—*Bull. Soc. Int.*, vol. 10, No. 99, p. 274 (I.).

ANON.—The Stanley-Kelly Alternate-Current Motor.—*Lum. El.*, vol. 49, No. 32, p. 277 (I.).

F. GUILBERT—Improvements in Direct-Current Machines.—*Lum. El.*, vol. 49, No. 33, p. 301 (I.).

T. MARCHER—Experiments on Unipolar Machines.—*E. T. Z.*, No. 32, 1893, p. 463 (I.).

L. BAUMGARDT—The Design of Notched Armatures.—*E. T. Z.*, 1893, No, 35, p. 497 (I.).

R. BAUCH—Regulation of Speed in Electrically-driven Machinery.—*Id.*, p. 499 (I.).

— BEHN-ESCHENBURG—The Theory of Non-Synchronous Motors.—*E.T. Z.*, 1893, No. 36, p. 519 (I.).

E. SCHULZ—Experimental and Theoretical Researches on Dynamo Machines.—*Id.*, p. 524, No. 37, p. 540.

F. GUILBERT—The Alternator of the "L'Eclairage Electrique" Company.—*Lum. El.*, vol. 50, No. 42, p. 101 (I.).

A. and J. BOISSONNAS—Output and Efficiency of Single-Phase Non-Synchronous Motors.—*Id.*, p. 109 (I.).

ANON.—Alioth's Improved Alternate-Current Motors.—*Id.*, p. 131 (I.).

P. BOUCHEROT—On the Theory of Rotating Field Machines.—*Lum. El.*, vol. 50, No. 43, p. 151 (I.).

E. KOLBEN—The Design of Alternate-Current Motors.—*E. T. Z.*, 1893, No. 40, p. 572, No. 41, p. 590 (I.).

MAGNETISM.

G. O. SQUIER—Electro-chemical Effects due to Magnetisation.—*Phil. Mag.*, vol. 35, No. 217, p, 473 (I.).

— BEHN-ESCHENBURG—A Simple Apparatus for Comparison of the Magnetic Qualities of various Kinds of Iron.—*E. T. Z.*, 1893, No, 23, p. 330 (I.).

P. CULMANN—On Magnetisation by very Small Forces.—*E. T. Z.*, 1893, No. 24, p. 345.

M. JÜLLIG—On a Phenomenon of Electro-magnetic Rotation.—*E. T. Z.*, 1893, No. 24, p. 345 (I.).

O. FRÖLICH—On the Theory of the Electro-Magnet.—*E. T. Z.*, 1893, No. 25, p. 365, No. 27, p. 387, No. 28, p. 401 (S. I.).

F. GUILBERT—On Compensation for Hysteresis.—*Lum. El.*, vol. 48, No. 24, p. 518 (I.).

E. BELTRAMI—On the Mathematical Theory of Magnetism.—*Beibl.*, vol. 17, No. 7, p. 668.

G. VAN DEVENTER—The Fixing of Magnetic Fields of Force.—*Beibl.*, vol. 17, No. 7, p. 668.

THOMPSON and Others—Magnetic Permeability of various Kinds of Iron.—*Beibl.*, vol. 17, No. 7, p. 668.

G. RÖSSLER—Researches on Magnetisation by very Large and very Small Forces.—*Beibl.*, vol. 17, No. 7, p. 669.

E. VILLARI—The Influence of Ordinary on Transverse Magnetisation in Iron and Steel—*Beibl.*, vol. 17, No. 7, p. 670.

M. CANTONE—Influence of Transverse Magnetisation on the Alterations of Resistance in Longitudinally Magnetised Iron and Nickel.—*Beibl.*, vol. 17, No. 7, p. 673.

A. VERNER—Attempted Explanation of Magnetic Rotation of the Plane of Polarisation according to Reusch's Experiments.—*Beibl.*, vol. 17, No. 8, p. 781.

H. EVERS—Recent Magnetic Researches.—*Id.*, p. 782.

E. HANNAUER—A Graphic Method of Solving Problems in Networks of Magnetic Circuits.—*E. T. Z.*, 1893, No. 36, p. 527 (I.).

A. DE TILLO—Values of the Magnetic Elements determined by the Polar Expedition to the Mouth of the Lena of the Imperial Russian Geographical Society —*C. R.*, vol. 47, No. 14, p. 457.

E. v. LOMMEL—Equipotential and Magnetic Force Lines.—*W. A.*, vol. 50, No. 10, p. 316 (I.).

E. v. LOMMEL—The Hall Phenomenon.—*Id.*, p. 320 (I.).

INSTRUMENTS AND MEASUREMENTS.

F. KOHLRAUSCH—On the Determination of Electrical Resistance by means of Alternating Currents.—*W. A.*, vol. 49, No. 6, p. 225.

F. HEERWAGEN—On the Temperature Coefficient of the Specific Inductive Capacity of Pure Water.—*W. A.*, vol. 49, No. 6, p. 272 (I.).

M. WIEN—A New Form of Induction Balance.—*W. A.*, vol. 49, No. 6, p. 306 (I.).

— BEHN-ESCHENBURG—Arrangement of Connections for Measurement of Pressure, Current, and Energy by means of a Siemens Dynamometer.—*E. T. Z.*, 1893, No. 22, p. 321 (I.).

ANON.—Electric Speed Counter for Ships' Shafts, made by Siemens & Halske.—*E. T. Z.*, No 23, p. 331 (I.).

P. CURIE—Condensers with Guard Rings, and Absolute Electrometers.—*Jour. de Phys.*, June, 1893, p. 265 (I.).

C. BOREL—Researches on the Specific Inductive Capacity of certain Biaxial Crystals.—*C. R.*, vol. 116, No. 26, p. 1509.

A. P. TROTTER—A New Photometer.—*Phil. Mag*, vol. 36, No. 218, p. 82 (I.).

F. PASCHEN—Highly Sensitive Astatic Thomson Mirror Galvanometer.—*Beibl.*, vol. 17, No. 7, p. 666.

E. W. LEHMANN—A Photometer.—*E. T. Z.*, No. 28, 1893, p. 407 (I.).

ANON.—Hunter's Electrostatic Ammeter.—*Lum. El.*, vol. 48, No. 26, p. 622 (I.).

ANON.—The Mosher Rheostat.—*Lum. El.*, vol. 49, No. 27, p. 31 (I.).

ANON.—Ericson Meter for Three-Wire Circuits.—*Lum. El.*, vol. 49, No. 28, p. 76 (I.).

ANON.—The Frölich Astatic Galvanometer.—*Lum. El.*, vol. 49, No. 28, p. 80 (I.).

J. L. ROUTIN.—A New Electricity Meter.—*Lum. El.*, vol. 49, No. 29, p. 108 (I.).

ANON.—Stevenson's Cable Tester.—*Lum. El.*, vol. 49, No. 29, p. 127 (I.)

ANON.—The Lattig Electric Semaphore Signal.—*Id.*, p. 130 (I.).

ANON.—The Hoydt Galvanometer.—*Lum. El.*, vol. 49, No. 30, p. 178 (I.).

ANON.—The Pattee Meter.—*Id.*, p. 179 (I.).

— LAGARDE—Note on the Electrical Resistance of Pure Copper Wire.—*Ann. Tel.*, vol. 20, No 3, p. 235.

— ARMAGNAT—The Carpentier Ohmmeter for Insulation Resistance.—*Bull. Soc Int.*, vol. 10, No. 98, p. 226 (I.).

L. LEJEUNE—Portable Testing Set.—*Bull. Soc. Int.*, vol. 10, No. 100, p. 314 (I.).

A. KOEPSEL—Permanent Magnet Ammeters by Siemens & Halske.—*Lum. El.*, vol. 49, No. 31, p. 227 (I.).

P. BOUCHEROT—The Analysis of Periodic Curves.—*Lum. El.*, vol. 49, No. 32, p. 251 (I.).

ANON.—Drake and Gorham's Electroscope.—*Lum. El.*, vol. 49, No. 34, p. 377 (I.).

K. KAHLE—Directions for Setting up a Clark Standard Cell.—*E. T. Z.*, No. 31, 1893, p. 447.

K. WILKENS—A New Iron Ammeter and Voltmeter by Hartmann & Braun.—*E. T. Z.*, No. 31, 1893, p. 446 (I.).

ANON.—Siemens & Halske's Telephonic Measuring Bridge.—*E. T. Z.*, No. 33, 1893, p. 478 (I.).

H. ZIELINSKI—The Thomson Lightning Arrester.—*E. T. Z.*, No. 34, 1893, p. 489 (I.).

ANON.—Mix and Genest's Conversation Counter for Telephone Exchanges.—*Jour. Tel.*, vol. 17, No. 9, p. 209 (I.).

E. VAN AUBEL—On the Electrical Resistance of Compressed Bismuth.—*Jour. de Phys.*, September, 1893, p. 407.

G. PELLISIER—The Breguet Telephone and Telegraph Calls.—*Lum. El.* vol. 49, No. 35, p. 405 (I.).

G. VAN DER W. DE FERNIG—Carbon Rheostat for Incandescent Lamps.—*Lum. El.*, vol. 49, No. 35, p. 407 (I.).

DEWAR and FLEMING—The Electrical Resistance of Metals and Alloys at Temperatures approaching the Absolute Zero.—*Phil. Mag.*, vol. 36, No. 220, p. 271 (I.).

E. VILLARI—A Modification of Thomson's Electrometer.—*Beibl.*, vol. 17, No. 8, p. 760.

C. CATTANEO—Negative Temperature Coefficient of the Electrical Conductivity in Ether Solutions.—*Id.*, p. 770.

M. KALLMANN—The Measurement of Resistance of Underground Cables, and of Large Faults.—*E. T. Z.*, 1893, No. 38, p. 545 (I.).

— LAGARDE—Note on the Resistance of Pure Copper Wire.—*Bull. Soc. Belge*, vol. 10, No. 5, p. 182.

ANON.—The Duncan Watt- and Ammeters.—*Lum. El.*, vol. 49, No. 40, p. 33 (I.).

ANON.—A New Weston Ammeter.—*Lum. El.*, vol. 49, No. 41, p. 76 (I.).

ANON.—The Borcherding Meter.—*Lum. El.*, vol. 50, No. 42, p. 128 (I.).

K. R. KOCH—A Simple Method of Measuring the Conductivity of Dielectric Liquids.—*W. A.*, vol. 50, No. 11, p. 482.

A. HEYDWEILLER—The Galvanic Measurement of Long Coils of Wire.—*W. A.*, vol. 50, No. 11, p. 571.

SIEMENS & HALSKE—Apparatus for Calibrating a Torsion Galvanometer.—*Beibl.*, vol. 17, No. 9, p. 848.

O. FRÖLICH—Measuring Triple-Current Power.—*E. T. Z.*, 1893, No. 40, p. 574 (I.).

K. STRECKER—Disturbances in Electrical Instruments due to Thermo-electric Effects.—*E. T. Z.*, 1893, No. 40, p. 575.

TELEGRAPHY AND TELEPHONY.

H. ZIELINSKI—Researches on the Working of Telegraphic Lightning Arresters.—*E. T. Z.*, 1893, No. 22, p. 319, No. 23, p. 333 (S. I.).

D. H. KEELEY—The Simplification of Quadruplex Working, and its Importance.—*E. T. Z.*, 1893, No. 25, p. 355 (I.).

ANON.—Telegraphs and Telephones in Switzerland in 1892.—*Jour. Tel.*, vol. 17, No. 6, p. 125 (S.).

ANON.—Telephonic Statistics for 1891.—*Jour. Tel.*, vol. 17, No. 6, p. 130.

ANON.—Statistics of the Portuguese Telegraphs, 1889.—*Jour. Tel.*, vol. 17, No. 6, p. 138.

P. HOHO—Different Systems of Telautography.—*Lum. El.*, vol. 48, No. 22, p. 416.

E. ZETSCHE—Automatic Transmitter of Messrs. Peyer, Favarger, & Co.—*Lum. El.*, vol. 48, No. 22, p. 419 (I.).

E. ZETSCHE—The different Systems of Working in Electric Telegraphy.—*Jour. Tel.*, vol. 17, No. 7, p. 149, No. 8, p. 173.

ANON.—Telegraphy in the British Indies in 1891-1892. Telegraphy in Switzerland, 1892.—*Jour. Tel.*, vol. 17, No. 7, p. 152.

ANON.—The Meyer Quadruple Contact Telephone.—*Lum. El.*, vol. 49, No. 28, p. 77 (I.).

ANON.—The Anders & Kottgen Domestic Telephone.—*Lum. El.*, vol. 49, No. 29, p. 128 (I.).

ANON.—Volker's System of Telephonic Connection for Short Distances.—*Id.*, p. 129 (I.).

ANON.—Bell Telegraphic Key.—*Lum. El.*, vol. 49, No. 30, p. 179 (I.).

E. MASSIN—Note on the Comparative Merits of various Types of Lightning Arresters.—*Ann. Tel.*, vol. 20, No. 3, p. 242 (I.).

ANON.—Measurement of the Duration of Telephonic Conversations.—*Ann. Tel.*, vol. 20, No. 3, p. 254.

ANON.—Brazilian Telegraphs in 1891.—*Jour. Tel.*, vol. 17, No. 8, p. 175.

ANON.—Telegraphs in the Dutch Indies in 1891.—*Id.*, p. 180.

C. R CROSS—The Amplitude of Vibration of the Telephone Diaphragm.—*Lum. El.*, vol. 49, No. 31, p. 232 (I.); *Beibl.*, vol. 17, No. 8, p. 787.

E. ZETSCHE—Multiple Telephone Switches.—*Lum. El.*, vol. 49, No. 34, p. 363 (I.).

ANON.—The Preece Submarine Cable.—*Lum El.*, vol. 49, No. 34, p. 378 (I.).

PREECE and BEESTON—Long-Distance Telephony.—*Phil. Mag.*, vol. 36, No. 219, p. 222.

ANON.—Telephonic Connection between Denmark and Sweden.—*E. T. Z.*, No. 33, 1893, p. 482.

H. WIETZ—The Gattinger Field-Telephone.—*E. T. Z.*, No. 34, 1893, p. 490, No. 35, p. 500 (I.).

E. Z.—The Averdiek Polarised Bell.—*Jour. Tel.*, vol. 17, No. 9, p. 202 (I.).

ANON.—Italian Telegraphs in 1891-92.—*Jour. Tel.*, vol. 17, No. 9, p. 203.

K. STRECKER and T. KARRASS—The Insulation Resistance of Ordinary Porcelain Double Insulators.—*E. T. Z.*, 1893, No. 35, p. 503.

C. GRAWINKEL—Recent Researches on the Disturbing Influence of a Bare Middle Wire on Telephone Circuits.—*E. T. Z.*, 1893, No. 38, p. 548.

J. ANIZAN—Telegraphy and Cryptography.—*Jour. Tel.*, vol. 17, No. 10, p. 221.

ANON.—Comparative Telegraphic Statistics for 1892.—*Id.*, p. 224.

ANON.—Telegraphs in Great Britain, 1892-93.—*Id.*, p. 233.

ANON.—Denison's Chemical Writing Telegraph.—*Lum. El.*, vol. 49, No. 40, p. 32 (I.).

ANON.—Method of Increasing Speed of Transmission in Long Submarine Cables.—*Lum. El.*, vol. 49, No. 41, p. 75 (I.).

M. KALLMANN—The Question of Bare Middle Wires and Telephone Circuits.—*E. T. Z.*, 1893, No. 40, p. 573.

O. CANTER—Influence of the Capacity of the Junction Transmitter on Speaking in Coupled Lines.—*E. T. Z.*, 1893, No. 41, p. 591.

STATIC AND ATMOSPHERIC ELECTRICITY.

K. WESENDONCK—On Electric Discharges.—*W. A.*, vol. 49, No. 6, p. 295.

E. C. RIMINGTON—Luminous Discharges in Electrodeless Vacuum Tubes.—*Phil. Mag.*, vol. 35, No. 217, p. 506 (I.).

E. GOLDSTEIN—On a Property of the Anodes of Geissler's Tubes.—*Phil. Mag.*, vol. 35, No. 217, p. 538.

A. RIGHI—Arrangement of Apparatus for Hertz Experiments.—*Lum. El.*, vol. 48, No. 24, p. 508 (I.).

J. BLONDIN—Leakage of Electricity through Gases.—*Lum. El.*, vol. 48, No. 24, p. 511.

W. H. HARVEY and H. BIRD—Some Notes on Brush Discharge in Gases.—*Phil. Mag.*, vol. 36, No. 218, p. 45 (I.).

L. ZEHNDER—The Objective Representation of Hertz's Experiments and the High-Pressure Accumulator.—*W. A.*, vol. 49, No. 7, p. 549 (I.).

E. BRANLY—Discharge of Electricity under the Influence of Daylight.—*Jour. de Phys.*, July, 1893, p. 300 (I.).

A. RIGHI—Hertz Researches with Oscillations of Short Wave-Length.—*Lum. El.*, vol. 48, No. 26, p. 601 (I.).

J. BLONDIN—The Luminous Intensity of Discharges in Rarefied Gases.—*Lum. El.*, vol. 49, No. 28, p. 56 (I.).

— DEVAUX—A New Experiment in Electric Discharges.—*Ann. Tel.*, vol. 20, No. 3, p. 252.

P. JANET—On Electrical Oscillations of Average Frequency.—*Jour. de Phys.*, August, 1893, p. 337 (I.).

ANON.—On Residual Charges.—*Lum. El.*, vol. 49, No. 37, p. 540 (I.).

J. BLONDIN—On the Pressure in the Interior of Polarised Dielectrics.—*Lum. El.*, vol. 49, No. 38, p. 551.

J. WIMSHURST—On a New Form of Influence Machine.—*Phil. Mag.*, vol. 36, No. 220, p. 264 (I.).

W. R. PIDGEON—An Influence Machine.—*Id.*, p. 267 (I.).

A. L. HOLZ—Electric Excitation by Spray.—*W. A.*, vol. 50, No. 9, p. 147.

H. FRITZ—Evolution of Heat in Glass under Electrical Polarisation.—*Beibl.*, vol. 17, No. 8, p. 762.

F. Pockels—Alterations of Volume and Form due to Electric or Magnetic Polarisation.—*Id.*, p. 766.

K. Wesendonck—On Discharge from Points.—*W. A.*, vol. 50, No. 11, p. 470.

J. Tuma—Measurement of Atmospheric Electricity from a Balloon.—*Beibl.*, vol. 17, No. 9, p. 855.

J. J. Thomson—On the Effect of Electrification and Chemical Action on a Steam Jet, and of Water Vapour on the Discharge of Electricity through Gases.—*Phil. Mag.*, vol. 36, No. 221, p. 313.

J. Trowbridge—On the Oscillations of Lightning Discharges and of the Aurora Borealis.—*Phil. Mag.*, vol. 36, No. 221, p. 343 (I.).

ELECTRO-CHEMISTRY.

Anon.—Electric Deposition of Metals on Glass, Porcelain, &c.—*E. T. Z.*, 1893, No 23, p. 333.

J. Garnier—The Effect of Electricity on the Carburation of Iron.—*C. R.*, vol. 116, No. 25, p. 1449.

G. H. Zahn—On the Processes which take place at the Boundary between Two Solutions of different Concentration when a Current is passed.—*Phil. Mag.*, vol. 36, No. 218, p. 151.

S. Lussana—Experimental Researches on Thermo-electric Effects between Electrolytes.—*Beibl.*, vol. 17, No. 7, p. 662.

N. Bunge—On Electrolysis of the Halogen Derivatives of Organic Acids.—*Beibl.*, vol. 17, No. 7, p. 664.

Anon.—Electrical Formation of Carborundum—Acheron Process.—*Lum. El.*, vol. 48, No. 26, p. 621 (I.).

Anon.—Siemens and Obach Dry Cell.—*Lum. El.*, vol. 48, No. 26, p. 621 (I.).

Anon.—The Craney Electrolyser.—*Lum. El.*, vol. 49, No. 27, p. 30 (I.).

Anon.—The Waite Electrolytic Diaphragm.—*Id.*, p. 30.

Anon.—The Hoetzer Cell.—*Id.*, p. 32 (I.).

G. Richard—Aluminium and its Electro-Metallurgy.—*Lum. El.*, vol. 49, No. 28, p. 67 (I.).

Anon.—Electrolysis of the Alkaline Chlorides—Fitzgerald Process.—*Lum. El.*, vol. 49, No. 28, p. 80.

Anon.—Electrolysis of Sodium—Clay Bull Process.—*Lum. El.*, vol. 49, No. 29, p. 129 (I.).

Anon.—Electrolytic Manufacture of Copper Wire and Strip—Sanders Process.—*Id.*, p. 130 (I.).

— Hillairet—Action of Electricity on the Carburation of Iron.—*Bull. Soc. Int.*, vol. 10, No. 100, p. 324.

Anon.—Electrolysis of Sodium Chloride—Craney Process, 1893.—*Lum. El.*, vol. 49, No. 34, p. 378 (I.).

Anon.—A New Bleaching Process: Electrolytic Preparation of Aniline Dyes; Electric Deposition of Rhodium.—*E. T. Z.*, No. 34, 1893, p. 494.

H. Moissan—Action of the Electric Arc on the Diamond, Amorphous Boron, and Crystallised Silicon.—*C. R.*, vol. 117, No. 13, p. 423.

ANON.—The Electrolysis of Alkaline Salts—Castner Process.—*Lum. El.*, vol. 49, No. 35, p. 425 (I.).

ANON.—Electro-Metallurgy of Gold.—*Lum. El.*, vol. 49, No. 36, p. 479.

ANON.—The Tommassi Electrolyser.—*Lum. El.*, vol. 49, No. 37, p. 526 (I.).

W. HITTORF—The Electro-motive Forces of Galvanic Combinations.—*Beibl.*, vol. 17, No. 8, p. 771.

F. OETTEL—Electrolysis of Copper Sulphate.—*Id.*, p. 774.

R. MALAGOLI—On the Theory of Electrolysis with Alternate Currents.—*Id.*, p. 775.

W. OSTWALD—On the Thermal Chemistry of Ions.—*Id.*, p. 777.

W. OSTWALD—The Dissociation of Water.—*Id.*, p. 779.

ANON.—The Shrewsbury and Dobell Battery.—*Lum. El.*, vol. 49, No. 41, p. 78 (I.).

ANON.—Moissan's Preparation and Properties of Silicide of Crystallised Carbon (or Carborundum).—*Lum. El.*, vol. 50, No. 42, p. 133 (I.).

— ATCHESON—Carborundum.—*Id.*, p. 135 (I.).

H. EBERT—The Heat of Dissociation in Electro-chemical Theory.—*W. A.*, vol. 50, No. 10, p. 255.

R. J. HOLLAND—The Conductivity of Solutions of Copper Chloride.—*Id.*, p. 349 (I.).

F. KOHLRAUSCH—The Speed of Electric Ions.—*W. A.*, vol. 50, No. 11, p. 385.

SMITH and SALTAR—Electrolytic Decompositions.—*Beibl.*, vol. 17, No. 9, p. 847.

ACCUMULATORS.

C. BRÜGGEMANN—The Formation of Gas in the Charging of Accumulators.—*E. T. Z.*, 1893, No. 24, p. 341 (I.).

K. STRECKER and T. KARRASS—An Improved Method of Soldering the Electrodes of Accumulators.—*E. T. Z.*, 1893, No. 24, p. 344.

F. STREINTZ—Contributions to the Theory of the Secondary Cell.—*W. A.*, vol. 49, No. 7, p. 564 (I.).

ANON.—The Boese Cell.—*Lum El.*, vol. 49, No. 27, p. 32.

ANON.—The Rosenthal and Doubleday Cell.—*Lum. El.*, vol. 49, No. 30, p. 180 (I.).

ANON.—The Lehmann 1893 Cell.—*Id.*, No. 32, p. 274.

ANON.—The Hofmann Cell, 1893.—*Lum. El.*, vol. 49, No. 34, p. 379 (I.).

ANON.—The Smith Cell, 1892.—*Id.*, vol. 50, No. 42, p. 128 (I.).

ANON.—The Müller Cell, 1892.—*Id.*, p. 129 (I.).

H. STRECKER—The Theory of the Secondary Cell.—*E. T. Z.*, 1893, No. 41, p. 592.

THEORY.

E. PRINGSHEIM—Kirchhoff's Law, and the Radiation of Gases.—*W. A.*, vol. 49, No. 6, p. 347.

D. KORDA—On Doubling the Periodicity of an Alternating Current, and the Measurement of Difference of Phase.—*E. T. Z.*, 1893, No. 23, p 329.

D. KORDA—Trebling the Periodicity of Sine Currents.—*E. T. Z.*, 1893, No. 25, p. 355.

— VASCHY—An Attempt at a New Theory of Electrostatics.—*Ann. Tel.*, vol. 20, No. 3, p. 205; *C. R.*, vol. 116, No. 23, p. 1286; *Jour. Tel.*, vol. 17, No. 9, p. 197.

— VASCHY—On a General Property of Electric and Magnetic Fields.—*Ann. Tel.*, vol. 20, No. 3, p. 230; *C. R.*, vol. 116, No. 25, p. 1437.

— CLAVENAD—Observations on the Theory of Electric Magnitudes.—*Lum. El.*, vol. 48, No. 23, p. 467, No. 25, p. 551.

A. HESS—Elimination of Superior Harmonics in Periodic Currents, and the Production of Perfectly Sinusoidal Currents.—*Lum. El.*, vol. 48, No. 24, p. 501 (I.).

O. LODGE—The Foundations of Dynamics.—*Phil. Mag.*, vol. 36, No. 218, p. 1.

L. BOLTZMANN—On the Methods of Theoretical Physics.—*Phil. Mag.*, vol. 36, No. 218, p. 37.

F. Y. EDGEWORTH—Exercises in the Calculation of Errors.—*Phil. Mag.*, vol. 36, No. 218, p. 98.

A. ELSAS—On the Theory of Electric Oscillations in Wires.—*W. A.*, vol. 49, No. 7, p. 487.

E. v. LOMMEL—Equipotential and Magnetic Lines of Force.—*W. A.*, vol. 49, No. 7, p. 539 (I.).

C. COLTRI—The Distribution of Current in Conducting Networks.—*E. T. Z.*, No. 30, 1893, p. 425 (I.).

E. MERCADIER—On the General Relations between the Fundamental Laws of Electricity and Magnetism, and their Consequences.—*Jour. de Phys.*, July, 1893, p. 289; *Ann. Tel.*, vol. 20, No. 3, p. 189.

C. RAVEAU—Recent Researches on the Electro-magnetic Theory of Light.—*Lum. El.*, vol. 49, No. 27, p. 7, No. 30, p. 169, No. 32, p. 263, No. 33, p. 320 (S.).

J. BLONDIN—The Conservation of Energy: Application to Electric Phenomena.—*Lum. El.*, vol. 49, No. 29, p. 101.

R. MALAGOLI—Contribution to the Theory of Electrolysis by Alternate Currents.—*Jour. de Phys.*, August, 1893, p. 370.

J. BLONDIN—Determination by Electrical Methods of the Mechanical Equivalent of Heat.—*Lum. El.*, vol. 49, No. 31, p. 201.

J. BLONDIN—Recent Researches on the Magnetic Rotation of Polarised Light.—*Lum. El.*, vol. 49, No. 34, p. 351, No. 35, p. 415, No. 36, p. 466, No. 37, p. 514, No. 38, p. 569, No. 39, p. 617.

A. BLONDEL—On the Measurement of the Mean and Instantaneous Values of Self-Induction in Circuits of Variable Permeability.—*Lum. El.*, vol. 49, No. 34, p. 373 (I.).

A. MCAULAY—Notes on a Modification of Maxwell's Electrical Theory.—*Phil. Mag.*, vol. 36, No. 219, p. 175.

G. M. MINCHIN—The Magnetic Field close to the Surface of a Wire Conveying an Electric Current.—*Phil. Mag.*, vol. 36, No. 219, p. 201 (I.).

P. DRUDE—On the Theory of Magneto-optical Phenomena.—*W. A.*, vol. 49, No. 8, p. 690.

J. PULUJ—The Working of Sinusoidal Electro-motive Forces in a Conductor having Self-Induction.—*E. T. Z.*, No. 32, 1893, p. 466.

C. P. STEINMETZ—The Electro-magnetic Constants of Parallel Wires.—*E. T. Z.*, No. 33, 1893, p. 476.

P. BOUCHEROT—Methods of Measuring Self-Induction.—*Lum. El.*, vol. 49, No. 36, p. 451.

J. TEICHMÜLLER—Distribution of Current in Conducting Networks.—*E. T. Z.*, 1893, No. 37, p. 537 (I.).

S. PAGLIANI—The Specific Inductive Capacities and Indices of Refraction of various Substances.—*Lum. El.*, vol. 49, No. 41, p. 81.

S. PAGLIANI—Equations of Refraction of Light.—*Lum. El.*, vol. 50, No. 42, p. 140.

C. P. STEINMETZ—The Utilisation of Complex Quantities in Electrotechnical Work.—*E. T. Z.*, 1893, No. 42, p. 597 (I.).

VARIOUS.

A. KUNDT—The Hall Phenomenon in Iron, Cobalt, and Nickel.—*W. A.*, vol. 49, No. 6, p. 257 (I.).

J. DANIEL—On Phenomena of Galvanic Polarisation in Thin Metal Partitions in a Voltameter.—*W. A.*, vol. 49, No. 6, p. 281.

T. DES COUDRES—Non-Polarising Electrolytic Cells under the Influence of Centrifugal Force.—*W. A.*, vol. 49, No. 6. p. 284 (I.).

P. KLEMENCIC—On the Disengagement of Heat occurring when Electrical Vibrations are Transmitted through Wires.—*Phil. Mag.*, vol. 35, No. 217, p. 537.

C. POLLAK—On a New Method of Transforming Alternate into Direct Currents. —*C. R.*, vol. 116, No. 26, p. 1512.

A. RENAULT—M. Ditte's Researches on the Leclanché Cell.—*Lum. El.*, vol. 48, No. 22, p. 416 (S. I.).

P. F. MOTTELAY—A History of Electrical Progress.—*Lum. El.*, vol. 48, No. 22, p. 424, No. 24, p. 522, No. 25, p. 565, No. 26, p. 609, No. 28, p. 71, No. 34, p. 366, No. 35, p. 417 (S.).

E. CHAPEAU—A New Electric Bell.—*Lum. El.*, vol. 48, No. 24, p. 520 (I.).

E. GENGLAIRE—A Simple Primary Battery.—*Lum. El.*, vol. 48, No. 25, p. 574 (I.).

A. HESS—Insulators.—*Beibl.*, vol. 17, No. 7, p. 660.

R. ARNO—Rotating Electric Field, and Rotation produced by Electrostatic Hysteresis.—*Beibl.*, vol. 17, No. 7, p. 675.

J. VIOLLE—Luminosity of certain Refractory Substances in the Electric Arc.—*C. R.*, vol. 117, No. 1, p. 33.

A. D'ARSONVAL—A New Method of Electrification of Living Beings; and a Means of Measuring High-Frequency Magnetic Fields.—*C. R.*, vol. 117, No. 1, p. 34 (I.).

H. BAGARD—The Electric Conduction of Heat in Electrolytes.—*C. R.*, vol. 117, No. 2, p. 97 (I.).

G. Pellissier—The Invention of the Electric Battery.—*Lum. El.*, vol. 48, No. 26, p. 628.

Anon.—Carstarphen Cigar Lighter.—*Lum. El.*, vol. 49, No. 27, p. 31 (I.).

Anon.—The Manufacture of Mica Plates.—*Lum. El.*, vol. 49, No. 28, p. 77 (I.).

Anon.—A Fire Alarm.—*Id.*, p. 78 (I.).

Anon.—Lead-Covering Machine for Cables.—*Id.*, p. 82 (I.).

Anon.—Electro-Optics.—*Id.*, p. 95.

W. von Turin—Influence of Magnetic and Electric Forces on the Concentration of Solutions.—*Lum. El.*, vol. 49, No. 33, p. 337.

H. Ebert—A Mechanical Model for the Illustration of the Laws of Induction.—*W. A.*, vol. 49, No. 8, p. 642 (I.).

H. Ebert—Electrical Oscillations of Molecular Groups.—*W. A.*, vol. 49, No. 8, p. 651.

L. Zehnder—On Reflection and Resonance in Hertzian Oscillations.—*W. A.*, vol. 49, No. 8, p. 724 (I.).

Anon.—The Chicago Exhibition.—*E. T. Z.*, No. 32, 1893, p. 461, No. 33, p. 472, No. 34, p. 485, No. 41, p. 581, No. 42, p. 599 (S. I.).

— Mergier—An Optical Method of Studying Electric Waves and Measuring their Length.—*C. R.*, vol. 117, No. 13, p. 435.

A. Righi—On the Plane of Polarisation of Hertz Oscillations.—*Lum. El*, vol. 49, No. 35, p. 401, No. 41, p. 51.

Sarasin and De la Rive—Interference of Electric Waves.—*Lum. El.*, vol. 49, No. 36, p. 485, No. 37, p. 533, No. 38, p. 586 (I.).

A. Garbasso—On the Reflection of Electric Force Rays.—*Id.*, p. 489.

A. A. C. Swinton—Experiments with High-Frequency Discharges.—*Phil. Mag.*, vol. 36, No. 220, p. 300 (I.).

Ebert and Wiedemann—Luminous Phenomena in Rarefied Electrodeless Spaces under the Influence of Quickly Changing Electric Fields.—*W. A.*, vol. 50, No. 9, p. 1, No. 10, p. 221 (I.).

E. Englisch—Thermo-electric Researches.—*Id.*, p. 88 (I.).

F. Braun—The Physical Meaning of Thermo-Electricity.—*Id.*, p. 111.

Kohlrausch and Rose—The Solubility of Sparely Soluble Substances in Water determined by the Electrical Conductivity of the Solutions.—*W. A.*, vol. 50, No. 9, p. 118.

A. Kleiner—Heat Produced by Dielectric Polarisation.—*Id.*, p. 138.

A. Franke—The Specific Inductive Capacity of Fluids with relation to Temperature, and the Mossotti-Clausius Formula.—*Id.*, p. 163.

Klemencic and Czermak—On the Interference of Electric Waves in Air.—*Id.*, p. 174.

R. Weber—On Specific Inductive Capacity.—*Beibl.*, vol. 17, No. 8, p. 760.

Anon.—The Fein Transformer for Electrical Cauterisation.—*E. T. Z.*, 1893, No. 39, p. 563 (I.).

J. Colin—Influence of the Surface Condition of Platinum Electrode on its Initial Capacity for Polarisation.—*C. R.*, vol 47, No. 14, p. 459.

L. Houllevigue—The Electrical Transmission of Heat.—*C. R.*, vol. 47, No. 16, p. 516.

R. BLONDLOT—Measurement of the Speed of Propagation of an Electrical Disturbance along a Copper Wire, by a Method independent of Theory.—*C. R.*, vol. 47, No. 17, p. 543 (I.).

C. RAVEAU—The Propagation of Light in Metals.—*Lum. El.*, vol. 49, No. 40, p. 16, No. 41, p. 60; vol. 50, No. 42, p. 119 (I.).

ANON.—Ferranti Cables, 1892.—*Lum. El.*, vol. 50, No. 42, p. 130 (I.).

W. DE FONVIELLE—Electricity at the Progress Exhibition.—*Id.*, No. 43, p. 166.

R. J. HOLLAND—Alteration of the Conductivity of a Solution by Addition of Small Quantities of a Non-Conductor.—*W. A.*, vol. 50, No. 10, p. 261 (I.).

J. KLEMENCIC—Contribution to the Knowledge of Absorption and Distribution of Electric Oscillations in Wire Networks.—*W. A.*, vol. 50, No. 11, p. 456 (I.)

G. VINCENTINI—Influence of Atmosphere on the Resistance of Contacts.—*Beibl.*, vol. 17, No. 9, p. 843.

LORD KELVIN—On the Piezo-electric Property of Quartz.—*Phil. Mag.*, vol. 39, No. 221, p. 331 (I.).

LORD KELVIN—On a Piezo-electric Pile.—*Id.*, p. 342.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

INDEX TO VOL. XXII.

1893.

ABSTRACTS.